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THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

ROBERT JAMESON,

REGIUS PROFESSOR OF NATURAL HISTORY, LECTURER ON MINERALOGY, AND KEEPER OF THE MUSEUM IN THE UNIVERSITY OF EDINBURGH;

Fellow of the Royal Societies of London and Edinburgh; Honorary Member of the Royal Irish Academy; of the Royal Society of Sciences of Denmark; of the Royal Academy of Sciences of Berlin; of the Royal Academy of Naples; of the Geological Society of France; Honorary Member of the Asiatic Society of Calcutta; Fellow of the Royal Linnean, and of the Geological Societies of London; of the Royal Geological Society of Cornwall, and of the Cambridge Philosophical Society; of the Antiquarian, Wernerian Natural History, Royal Medical, Royal Physical, and Horticultural Societies of Edinburgh; of the Highland and Agricultural Society of Scotland; of the Antiquarian and Literary Society of Perth; of the Statistical Society of Glasgow; of the Royal Dublin Society; of the York, Bristol, Cambrian, Whitby, Northern, and Cork Institutions; of the Natural History Society of Northumberland, Durham, and Newcastle; of the Imperial Pharmaceutical Society of Petersburgh; of the Natural History Society of Wetterau; of the Mineralogical Society of Jena; of the Royal Mineralogical Society of Dresden; of the Natural History Society of Paris; of the Philomathic Society of Paris; of the Natural History Society of Calvados; of the Senkenberg Society of Natural History; of the Society of Natural Sciences and Medicine of Heidelberg; Honorary Member of the Literary and Philosophical Society of New York; of the New York Historical Society; of the American Antiquarian Society; of the Academy of Natural Sciences of Philadelphia; of the Lyceum of Natural History of New York; of the Natural History Society of Montreal; of the Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanic Arts; of the Geological Society of Pennsylvania; of the Boston Society of Natural History of the United States; of the South African Institution of the Cape of Good Hope; Honorary Member of the Statistical Society of France; Member of the Entomological Society of Stettin, &c. &c. &c.

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THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

On Isomeric Transmutation, and the Views recently published concerning the compound nature of Carbon, Silicon, and Nitrogen. By GEORGE WILSON, M.D., Lecturer on Chemistry, Edinburgh. Communicated by the Author.*

I propose, in the following Memoir, to offer some observations on the views recently published by Dr Samuel Brown, Mr Knox, and Mr Rigg, concerning the compound nature of silicon, nitrogen, and carbon. Before entering, however, at any length on the discussion of these, I would consider, very briefly, some points connected with the general question of the simplicity and unity of matter.

The great majority of chemists acknowledge the existence of some 55 simple or elementary substances. These are declared to be simple, not in virtue of any test of simplicity which the chemist has discovered and applied to them, but solely because they resist the decomposing or modifying action of all the forces which are, or at least are known to be, at man's disposal. The chemist, as it were, begins with the globe itself, and breaks it down into some thousand organic and inorganic compounds; these, in their turn, he resolves into some hundred less complex substances; and the latter, last of all, into the 55 bodies which are called simple. Here his analysis, in the meanwhile, has ended; all the forces which are at his command, for the modification of matter, having been spent in vain on these refractory substances. The single and combined agencies of heat, light, electricity, magnetism, mechanical pressure, and the like, have been directed in innumerable ways against them. But they have emerged from every trial, except those we are soon to consider, without betraying any sign of non-simplicity, or unfolding, if they are compound, the hidden secret of their true nature.

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* This memoir contains nearly verbatim the substance of a lecture delivered on 6th May 1844.

On the negative evidence of this insusceptibility of decomposition, the residual undecomposed bodies have been termed simple or elementary: they are the visible elements out of which all things are made. It cannot be denied, however, that in the minds of many, the term "*simple*" has passed for something more than the expression of "*hitherto undecomposed*," and has been accepted as fully equivalent to essentially "*indecomponible*." But it would be doing injustice to the majority of chemists, to affirm that they have not employed the word "*elementary*" in its restricted and negative meaning, and have been willing to acknowledge the possible compositeness of all the so-called simple bodies. I refer to this the more particularly, that, in a curious volume recently published by Professor Low, exception has been taken to the maxim acknowledged among chemists, that a body should be considered simple till it can be shewn to be compound, and the opposite opinion advocated, "that a body is to be regarded as compound, when we are not able to prove it to be simple."* Mr Low is at great pains to shew, that the maxim he objects to "is unsound, and is arrived at, not 'by the just logic of Chemical Philosophy,' but by a chemical dogma which ought, long ere now, to have been banished from the science into which it has been introduced."† Every chemist, however, will smile at this correction; for the proposition that all bodies, which cannot be resolved into something less complex than themselves, shall be accepted as simple, is quite accurate, and of much practical value, when taken in the sense in which he uses it. The simplicity of the so-called elementary bodies, is not affirmed to be *intrinsic*, *essential*, or *absolute*, but only to exist in relation to the decomposing or modifying forces which chemistry supplies. It remains competent to whomsoever chooses, to affirm, on the grounds of analogy, probability, direct experiment, or whatever else may seem to warrant it, that any or all of them are not simple substances: all that the chemist contends for is, that, tested by their power to resist the weapons and agents he can direct against them, they preserve their simplicity.

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* An Inquiry into the Nature of the Simple Bodies of Chemistry, by David Low, F.R.S.E., &c. p. 9.

† Ibid. pp. 11, 12.

Professor Low would have the chemical elements included among compound substances, because it violates the law of continuity in nature, to suppose some 55 bodies simple, whilst all the rest are compound. This may or may not be true; but it could do no service to term the elements *compound*, unless we were prepared immediately to follow up the statement, by shewing of what they are compounded. Such a proposition is consistent enough on Mr Low's part, since he offers a scheme of their composite constitution, founded on certain hypothetical views; but it is not competent to the chemist. All the knowledge he possesses of the composition of bodies, has been obtained by decomposing, or combining them, or by transforming them without decomposition, into each other. According to the characters they have shewn, when thus treated, they have been named and classified in the order of their complexity, and so as to shew, within certain limits, the nature and number of their several components. But the elementary bodies being insusceptible of resolution into substances more simple than themselves, cannot be affirmed to be compound in the sense the other bodies the chemist considers, are; and it is not his office to discuss their complexity on other grounds than those afforded by their behaviour, when subjected to analytic, synthetic, or purely transformative forces.

Whilst, therefore, I fully sympathise with the speculative spirit that has led Mr Low to propose a scheme of elemental constitution, and differ from most of my fellow chemists, in believing that some such scheme will hereafter be realized in our laboratories, I dissent from him in thinking that the chemist has erred, in demanding that every undecomposed body shall be considered simple. The term residual, or residuary, might indeed be better than simple, as indicating more clearly *undecompounded* as distinguished from *indecomponible*; but, after all, that, or any other novel phrase is unnecessary, for the explicit sense in which the term *simple* is applied by the chemist, leaves the question of the *bona fide* simplicity of the elementary bodies quite open to discussion. How fully it does so, may best be gathered from the fact, that Sir H. Davy and Berzelius, two of the warmest advocates of the maxim I have been discussing, were the freest in speculating on the nature of the elementary bodies, and the foremost in endeavouring to decompose them.

Further, I would observe, before leaving the subject, that it is not necessary, or, indeed, desirable, in the discussion of many chemical problems, that the possibility of the elementary bodies being simple should be considered. The study of most of the properties of the suite of oxides of a metal, or of a series of organic compounds, would not be facilitated, especially to a beginner, by shaking his faith in the stability and unchangeability of their simplest components. The elementary bodies stand in truth, in relation to all the more complex substances into whose composition they enter, like arithmetical ciphers, possessing in regard to all numbers higher than their own a fixed value, unalterable by any discovery which may be made concerning the lower figures which make up them. Silicon may be a simple body, as many believe, or a modification of carbon, as Dr Brown supposes, or a compound of carbon, hydrogen, and oxygen, or of carbon and hydrogen, as Mr Low thinks probable. But whichever of these it be, if any, is as indifferent to the chemist, while ascertaining the proportion in which it combines with oxygen to form silica, and a multitude of its other relations, as it was to the builder of an Egyptian pyramid, whether the bricks he made use of, so long as they possessed the proper shape and weight, and coherence, consisted of clay alone, or of clay mixed with sand, or of clay and sand mingled with straw.

We are free then to speculate to the uttermost on the nature of the elementary bodies; and if we consider from what direction we are likely to obtain the means of lessening their number, we shall find that the hopes of chemists (*i. e.* of those who hope at all on the matter) are fixed at present on three different quarters, from any, or from all of which, the power to effect the desired reduction may come. One method open to the chemist, is analysis; another synthesis. The experiments I am about to notice illustrate the application of both; for the same researches which seem to Dr Brown to establish synthetically the compound nature of silicon, appear to Mr Knox to demonstrate analytically the compound nature of nitrogen. The third method is not so easily defined; it may be termed that of reduction, by mutual isomeric transmutation.

The application of analysis to the reduction of the elementary bodies is easily understood. Without any addition to the resources, in the way of agent and instrument, we at present possess, it may suffice to produce more remarkable decompositions than we have yet seen it effect. If Mr Rigg's and Mr Knox's experiments are confirmed, it certainly will. Moreover, we may anticipate the discovery of novel agents, or of new powers in those we are familiar with, as we have recently become aware of the presence of marvellous modifying forces in the sunbeam, and in light and heat from other sources, of the existence of which we had scarcely a suspicion ten years ago. We may farther expect greatly to improve our instruments, and thereby to increase enormously our power over matter. Not to speak of what we should effect, could we realize certain improvements which theory indicates as possible in our voltaic batteries, the simple discovery of a substance which would resist the action of very high temperatures as effectually as platina and fireclay do our ordinary furnace heats, would put in the chemist's hands a weapon for conquest of the highest value. Many of the bodies which appear at present, to use the quaint words of old Sir Thomas Browne, "to lie immortal in the arms of fire," might then be found susceptible of resolution into simpler forms of matter. The possibility of all this is so apparent, that it is needless to enlarge on it at greater length. Before passing from it, however, I would observe, that the attempts of chemists to decompose the so-called simple bodies, appear to me to have been hitherto too much directed against the naked elements themselves, and not upon them in a state of combination; and, further, to have too much implied an expectation, that their decomposition was to be brought about, by some successful violent effort to tear or force asunder their constituents. Hence, the uselessly large battery which Davy employed when he effected the decomposition of the alkalis. But the more we learn of molecular forces, the more we seem to become aware of the truth, that the simple reversal or neutralization of the affinities which bind the components of a body together, is all that is necessary to effect its decomposition; and that this may be as fully secured by the invisible action of a sunbeam, or the inappreciable influence of an electric current, as by the most gigantic galvanic battery, or a furnace heated seven times more than is wont.

Meanwhile, it remains to be acknowledged, that analysis hitherto has done nothing to lessen the number of elementary bodies; on the other hand, it has continually been adding to them. The ancients acknowledged but four,—air, earth, fire, and water; a later school had their three,—salt, sulphur, and mercury; and no class of chemists, down to the destruction of the Phlogiston School, acknowledged, so far as I am aware, as many as a dozen. Since the era of Lavoisier, we have been steadily increasing the list, till now we count 55. Sir H. Davy only altered the names of the elements with metallic bases, without abridging the roll by one; and since his death, several new bodies have been ranked among simple substances. The further result of analysis, whether with its present or with additional powers, may be of the same kind. The fifty-five elementary bodies may each be resolved into two, or three, or four, unlike, and for the time, indecomponible substances; so that the list of elements shall be doubled, tripled, or quadrupled. But though this may be the first effect, analogy and probability conspire unequivocally to assure us, that it will not be the ultimate result of a victorious analysis of matter. As we find the prevailing elements of the countless organic bodies we examine, to be the constantly recurring four, carbon, hydrogen, oxygen, and nitrogen; and as Davy found a common element, oxygen, in all the earths, so we may expect, if the so-called elements are really compound, to find the same bodies present in many. We can suppose all the metals proving to be compounds in different proportions of but two: fluorine, chlorine, bromine, and iodine, in the same way reduced to two; carbon, boron, silicon, and the other groups of simple bodies, in like manner diminished to two. In this way, or in some other, we may resolve all the elementary bodies, as Mr Low thinks we shall, into the two lowest on the atomic scale, carbon and hydrogen; or, descending further, identify them every one, as Mr Rigg thinks we should, with hydrogen; or, in the lowest deep, finding a lower still, pass beyond even hydrogen to the long dreamed of Ὕλη πρωτη, the *materia prima*, or material substratum and essence of all things.

The application of synthesis to the reduction of the list of elementary bodies, is not so obvious as that of analysis, but may, on the whole, be made manifest enough. We can conceive the possibility of its being discovered, that two of the lower metals, such as lead and copper, when fused together formed gold; and that, nevertheless, the compound should be of such a nature, as to resist the decomposing influence of every agent. In such a case, it would be possible to prove gold not to be a simple substance, by shewing our ability to compound it out of lead and copper, though we might for ever remain unable to establish the same point analytically, by resolving it into these metals. Should such a synthetic demonstration of the compound nature of one of the elements ever be obtained, it would prepare us for attacking the problem of their true nature, by endeavouring to compound them out of each other. There is nothing, however, in the present state of Chemistry to warrant the expectation of such a discovery being made; and it is not in this shape, but as one of the forms of the method of reduction by isomeric transmutation, that synthesis has been applied to the diminution of the list of elementary bodies.

I turn now, therefore, to the consideration of Isomerism. For a long period after the publication of the Atomic Theory, it was universally believed that the same elements could combine in the same proportion to form only one compound, and that difference in physical properties, such as hardness, solubility, specific gravity, &c., was always occasioned by difference at least in the proportion of ingredients, and in most cases by difference also in their nature; and this is still acknowledged as true in regard to the majority of substances; water, *e. g.* is the only body containing oxygen and hydrogen, in the proportion of eight parts by weight of the former to one of the latter; common salt is the only substance, with thirty-five parts of chlorine to twenty-three of sodium, and so with other compounds. But within the last few years many bodies have been discovered containing the same elements, in the same proportion, and yet differing in every physical and chemical property. A striking example of this may be found in a group of organic substances particularly referred to by Liebig in his Familiar Letters:—"A great class of bodies," says he, "known as the volatile oils, oil of turpentine, essence of lemons, oil of balsam of copaiba, oil of rosemary, of juniper, and many others, differing widely from each other in their odour, their medicinal effects, their boiling points, their specific gravity, &c. are exactly identical in composition; they contain carbon and hydrogen in the same proportions;"* viz. five atoms of carbon to four of hydrogen. Bodies which possess this peculiarity are termed, in relation to each other, *Isomeric* (from ἴσος, *equal*, and μέρος, *part*), which may be Englished equiproportional, and marks their possession of an equal proportion of the same elements. The unexpected discovery of this curious law, while it has shewn chemists that the greatest dissimilarity in the properties possessed by bodies may accompany the most perfect coincidence in proportional composition, has, at the same time, directly led to the conclusion, that the elementary bodies may form a group, or a series of groups, related to each other isomerically, or equiproportionally, as the volatile oils referred to, are. Who first detected the applicability of the law of Isomerism to the possible solution of the problem of the true nature of the chemical elements, I do not know; nor do I profess to offer any historical sketch of the progress of speculation on this subject. I need only mention, that three of our living chemists have published views on the possible Isomerism of the elementary bodies,—Professor Johnston[*] in 1837; Dr Samuel Brown,[†] and Professor Kane in 1841.[‡]

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* Familiar Letters on Chemistry, by Justus Liebig, pp. 47, 48.

Dr Brown's theory, which I consider first, as it is a scheme of transmutation by synthesis, is founded upon the existence of a class of isomeric bodies, in which, while the equiproportionality of identical elements occurs, the number of atoms combining to produce this, is different in each member of the group. Thus, there exists a series of compounds of carbon and hydrogen, containing these bodies in the proportion of atom to atom. This is satisfied in the lowest, which is termed *methylene*, by 2 atoms of carbon to 2 of hydrogen; in the next *olefiant gas*, by 4 to 4; in the third *oil gas*, by 8 to 8, and in a fourth *cetene*, by 32 to 32. The volatile oils referred to previously, form so far at least, a similar series; in them the elements are also carbon and hydrogen, in the proportion of five atoms of the former to four of the latter. In oil of citron this is doubled, or we have $C_{10}\ H_8$; in oil of cubebs tripled, or $C_{15}\ H_{12}$; in turpentine quadrupled, or $C_{20}\ H_{16}$.

Groups of isomeric bodies of this kind are supposed by Dr Brown to be formed by successive duplications or doublings. The lowest member of the series, by combining with itself, produces a first multiple; this, by uniting with itself a second; that, by combining with itself a third; and so on *ad infinitum*. It is not essential, however, to the truth of this view, that a full series of duplicate multiples should be shewn to exist, provided no body is found to manifest an unequivocal departure from the rule: the gaps which occur in the known series may be filled up by future discoveries. Dr Brown thinks he has established the truth of his view by experiment, in regard to the isomeric compounds of carbon and nitrogen, cyanogen and paracyanogen; the latter of which he believes to be produced by the former combining with itself. In like manner he represents the 55 so-called simple substances as a group, or a series of groups, of isomeric bodies, produced by the element of lowest atomic weight (which may be the lowest at present known to us *hydrogen*, or a lower and more truly elemental body), forming successive combinations in the way already mentioned, so as to reach from, or through hydrogen, which we call 1, up to gold which is 199 times higher. To prevent any mistake, I quote Dr Brown's own words:*—"This view of isomerism, and the relation of cyanogen to paracyanogen, is farther recommended by the consideration, that it affords a practical foundation for a likely hypothesis of the constitution of the so-called chemical elements, and points out the way in which such a hypothesis may be either established or overthrown by experimental observation. Let it be supposed that several of the elemental groups are so many series of isomeric forms, and it is at once to be inferred, that heat, electrolysis, and reagents, shall all be incapable of decomposing them, as has been found in the actual practice of the laboratories of modern Europe, by innumerable trials. If, to take one instance, sulphur (16 or 2) be an isomeric form of oxygen (8 or 1), which it as much resembles in chemical properties, as it is conformably contrasted with it in mechanical condition, it must be impossible to extract oxygen from it by any analytical force which has yet been discovered; and the only method in which it shall be possible to prove that such is the mutual relation of these two elements, shall be to have recourse to synthesis, and convert oxygen into sulphur. It is within the scope of this hypothesis that the various elements may be all isomeric forms of one truly elementary substance."

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[*] Report of the Seventh Meeting of the British Association, pp. 163–215.

[†] Transactions of the Royal Society of Edinburgh, vol. xv. pp. 165–176, and 229–246.

[‡] Elements of Chemistry, p. 377.

Dr Brown's scheme of elemental reduction may be termed one by isomeric synthetic transmutation. You will observe, that he supposes transmutation to take place only by synthesis, and in one direction; so that an element possessing a certain atomic weight, may form, by uniting with itself, another possessing a higher combining proportion; but the reverse cannot occur. Oxygen, which is eight, may double itself into sulphur, which is sixteen; but sulphur cannot halve itself into oxygen; carbon may quadruple itself into silicon, but silicon cannot quarter itself into carbon. All the other elements may be transmuted into gold, which has the highest atomic weight; for, in this respect, Dr Brown's views are strictly in accordance with those of the alchymists; but gold can be changed into none of them, and, if it suffer transmutation, must pass into some unknown new body possessing a higher combining proportion. I shall return immediately to the consideration of those experiments by which Dr Brown thought he had proved the truth of his view, so far as carbon and silicon were concerned. Meanwhile, I proceed, very briefly, to explain in what respect Professors Johnston's and Kane's schemes of elemental isomerism differ from that we have been discussing.

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* Trans. Royal Society, vol. xv. p. 176.

Mr Johnston's views are founded on the existence of a class of isomeric bodies not taken into consideration in Dr Brown's speculations. The members of certain isomeric groups possess not only the same proportion of elements, but likewise the same atomic weight. They are not multiples or submultiples of each other, like those already considered, but owe their difference in properties to the relative grouping of their molecules otherwise than by multiplication, or simple superaddition of the atoms on each other. We have a group of three such bodies in cyanuric acid, hydrated cyanic acid, and cyamelide, compounds of carbon, oxygen, and nitrogen. We have another in aldehyde, metaldehyde, and eltaldehyde; and a well-known pair in urea and cyanate of ammonia.* These isomerics possess the character of mutual convertibility: thus, in a group of three, which we may term A, B, C; A is convertible into B and C; B into A and C; C into A and B; and this without addition or subtraction of any of their constituent elements. Guided by these facts, Professor Johnston observes, that "the speculations of chemists in regard to the probable diminution of the number of received elementary bodies, have hitherto run only in the channel of decomposition. * * * * The idea of a possible *transformation* has hitherto hardly been thought of; and yet the doctrine of isomerism, rich already in its numerous discoveries, has shewn that any number of the received elementary bodies *may* be made up of the same elements united in the same proportion."[*] After some incidental remarks, he continues, "It may be, however, that the patient study and pursuit of the kindred classes of phenomena we have been considering, shall, in some brighter moment, shew that substances considered elementary are yet *mutually convertible* without decomposition;" and, again, "It may be, indeed, that all our *supposed* elementary bodies are in *reality* such, and therefore wholly beyond the resolving energy of electricity, or any other agent; and yet the study of their changes and reactions in the laboratory, in conformity, perhaps, with new views or modes of investigation, may, at some future period, so enlarge our dominion over the molecules, as shall cause them, at our bidding, to assume this or that arrangement—to appear with the properties of chlorine or iodine, of cobalt or nickel, of rhodium, iridium, or osmium."[†] Professor Johnston's view, it will be observed, is a wider one than Dr Brown's, inasmuch as it acknowledges a possible mutual convertibility of the elementary bodies; and, therefore, implies that transmutation may proceed in both directions of the atomic scale. Sulphur may become oxygen, as readily as oxygen sulphur; silicon carbon, as readily as carbon silicon; gold hydrogen, as hydrogen gold. Any one element, in short, may become any other, whatever be their atomic weights. This scheme might be termed, in opposition to Dr Brown's, a method of elemental reduction by mutual isomeric transmutation.

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* Liebig's Familiar Letters, pp. 49, 50.

Professor Kane's views are too slightly sketched, in his work on Chemistry, to enable us to judge exactly in what way he expects the elements to prove isomeric, and they were certainly formed with a knowledge of what Professor Johnston had written on the subject. But he has indicated, in a way neither of the other chemists referred to have done, some remarkable relations between the atomic weights of certain of the metals, which would strikingly accord with either of their theories of elemental isomerism.

I do not offer any opinion as to the relative probability of Dr Brown's and Professor Johnston's views; but it is impossible to help wishing that the latter chemist's scheme of Elemental Isomerism should prove the truer of the two. For Dr Brown supplies us with but a one-edged weapon for conquering nature, while Professor Johnston puts in our hands a two-edged sword, smiting both ways, and increasing twofold our power over matter.

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[*] Report of British Association, vol. vi. p. 211.

[†] *Ibid.* p. 212.

Meanwhile, Dr Brown is the only chemist who has had faith and courage enough to test the reality of Elemental Isomerism, by endeavouring to transmute one of the elements into another. This, he believes, he has succeeded in doing in the case of carbon and silicon. His experiments have been made upon certain compounds of the former body with nitrogen, which he subjected to various modifying processes; one general principle, however, runs through them all, which may be explained in a few words. By a special process, instituted for the purpose, or as a product of a general process for transmutation, he obtained paracyanogen, a body consisting of carbon and nitrogen, in the proportion of twelve parts of the former to fourteen (by weight) of the latter; or of two atoms of carbon to one of nitrogen. The atomic weight and exact constitution of this body are unknown, but Dr Brown, as we have already seen, supposes it to be a duplication of cyanogen, and, therefore, to contain four atoms of carbon to two of nitrogen. When this body is treated in various ways, of which the simplest, and the only one we need consider, is that of heating it out of contact with air, alone, or in contact with substances (such as platina or carbonate of potass) having a strong attraction for silicon, its two atoms of nitrogen, according to Dr Brown, pass away unchanged, and its four atoms of carbon combine together, and form silicon. To some, perhaps, the view intended, and its relation to the isomerism of confessedly compound bodies, will be clearer, if they suppose for the moment that carbon is a compound of two elements, which are united in it in certain proportions, and in the same ratio, but in a multiple four times higher in silicon.

The greater number of chemists refused to acknowledge that silicon was, or could be, produced from paracyanogen; and, joining issue with Dr Brown on this point, offered no opinion on his theory of the origin of the silicon which appeared in his experiments. There was one chemist, however, Mr G. J. Knox, who not only accepted Dr Brown's statements as true, so far, at least, as the appearance of silicon was concerned, but advocated the probability of such an occurrence; on grounds, however, quite opposed to those Dr Brown himself built upon. Mr Knox's views are unfortunately not known to us fully, although it is more than a year since they were laid before the Royal Irish Academy. Owing to a peculiarity in the mode of publishing its transactions followed by that Society, the paper has not yet been printed; and the only shape in which its contents have reached us is that of an imperfect and insufficient abstract in one of our own journals.* Mr Knox conceives that the nitrogen of the paracyanogen, and not its carbon, is the source of the silicon which appeared in Dr Brown's experiments. His own words are the following: after referring to certain experiments of Sir H. Davy, which seem to him to warrant the belief that nitrogen is a compound body, he says, "The latest experiments which bear upon this subject, and from which I received the idea which led me to this investigation, are those of Dr Brown, 'upon the Conversion of Carbon into Silicon,'—an explanation of phenomena which appears to me most unreasonable, and contrary to all chemical analogy: while the supposition of the carbon having reduced the nitrogen, is not only a simple, but an unavoidable conclusion to arrive at, if nitrogen be a compound substance. To determine, by experiment, the correctness or incorrectness of this idea, it were only necessary to reduce nitrogen by some other substance than charcoal; and should silicon result from its decomposition, the problem might be considered to be solved." †

Mr Knox then describes several experiments made with a compound of hydrogen, nitrogen, and potassium, heated in different ways with iron, in two of which silicon appeared, although no carbon was present. The compound Mr Knox employed, he terms the "ammonia-nitruret of potassium," by which I understand him to signify the amidide of potassium (KNH^2) of other authors. He rejects one of the two experiments where silica appeared, as inconclusive as to its anomalous production; and draws from the whole the following conclusion: "From these experiments, together with those of Sir H. Davy mentioned above, one might infer that nitrogen is either a compound of silicon and hydrogen, or of silicon, hydrogen, and oxygen; to determine which synthetically, a current of dry muriatic acid was passed over siliciuret of potassium," and the resulting gases examined. These were found to contain a variable but marked proportion of nitrogen; so that, so far as can be judged from the imperfect account we possess, Mr Knox seems to consider nitrogen a compound of silicon and hydrogen, and to believe that he formed it by the action of muriatic acid on siliciuret of potassium. He does not suppose, however, as some have imagined, that the nitrogen is *transmuted* into silicon; he believes that the former *yields*, but not that it *forms* the latter, in the way Dr Brown supposes that carbon forms silicon. Silicon, according to Mr Knox, *pre-exists* in nitrogen, along with hydrogen, or with hydrogen and oxygen, by combination with which it makes up the nitrogen. He supposes, accordingly, that, in Dr Brown's experiments, the nitrogen was the source of the silicon, and that the carbon was useful only by combining with and removing the non-siliceous element or elements of the nitrogen, and setting the silicon free; and he endeavours to establish this by shewing, that if the other conditions of Dr Brown's experiments were retained, but the carbon replaced by a metal such as potassium (or rather by potassium and iron), the production of silicon went on as well as if carbon had been there. His view, therefore, has the advantage of explaining Dr Brown's results as well as his own; whereas that gentleman's theory affords no explanation of Mr Knox's experiments.* The latter, moreover, professes not only to have decomposed nitrogen into silicon and hydrogen, but to have combined silicon and hydrogen into nitrogen; so that he offers both synthetic and analytic proof of the truth of his views. It is impossible, however, to judge of the value of Mr Knox's experiments, till we see them reported in full; and there is a hesitation in his view of the constitution of nitrogen, as to whether it contains oxygen or not, which might, and should have been removed by prolonged experiments, before he published on the question at all. Moreover, he determines nothing as to the quantitative constitution of nitrogen, which should surely have been the chief object of investigation, as soon as he saw reason to believe that nitrogen was not a simple body.

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* Chemical Gazette, September 1843.

† Ibid., p. 574.

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* In so far as Dr Brown refers the silicon which appeared in his experiments to carbon, his explanation will, of course, not apply to researches like those of Mr Knox, where silicon was found, though no carbon was present. He may fall back, however, on his general hypothesis, that the higher elements are isomeric forms of the lowest, and affirm that the hydrogen of the ammonia-nitruret of potassium was transmuted, mediately through carbon, or immediately into silicon. When the text was written, I was not aware that Dr Brown had explained

As to the relative probability of the rival theories of the origin of the silicon, which appeared when paracyanogen was subjected to Dr Brown's processes, it is impossible at present to give a decision. I have repeated none of Mr Knox's experiments, and it would be presumptuous in me to criticise his results; but I devoted the greater part of last winter, along with my friend Mr John Crombie Brown, to the repetition of Dr S. Brown's processes for the transmutation of carbon into silicon, and I am free to offer an opinion on their value. Those who wish to know in detail the results my colleague and myself arrived at, will find them in the fifteenth volume of the Transactions of the Royal Society of Edinburgh.* Our general conclusions may be stated in a word.

We were able to confirm Dr Brown's phenomenal results thus far, that we obtained silicon in several of our experiments, in circumstances which seemed, to myself at least, to preclude the possibility of its being derived as an impurity or accidental ingredient, from the vessels or materials, or reagents made use of. The quantity was always much less, than by Dr Brown's hypothesis it should have been, and much less than he himself procured; in many experiments, moreover, no silicon was obtained at all. So far, however, as this scanty and precarious

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Mr Knox's results in this way. His hypothesis affords no explanation of the latter gentleman's synthetic experiments on the formation of nitrogen from silicon and hydrogen.

* Pp. 547–559.

appearance or production of silicon is concerned, we can authenticate Dr Brown's results, but no further. Some misapprehension, I believe, exists on this subject, and I am anxious it should continue no longer. I took the most public opportunity that was open to me last autumn,* of declaring my confident expectation, that a repetition of Dr Brown's processes would establish the truth of his theory; and I owe it to myself, still more to those I induced by my representations to advocate his cause, but above all to the interests of science, which must be hindered in its progress by the confusion of doubtful with certain knowledge, to take as public an opportunity of saying, that Dr Brown's processes have not, in my hands, yielded proof of the transmutability of carbon into silicon. I have further come to the conclusion, that they are too imperfect to establish the truth of that proposition in the hands of any one; and that there exists at present no evidence, in the way of demonstration by experiment, to satisfy a chemist, that carbon or any other element has ever suffered transmutation.

A peculiar difficulty attends the reception of the proposition, that carbon is transmutable into silicon; a difficulty which to many chemists seems insurmountable, and which has not been provided for by Dr Brown in any of his papers, although he was aware of its existence. It results from the irreconcilability of the atomic weights of carbon and silicon, the former of which is 6, the latter 22·22. According to Dr Brown, an atom of silicon consists of 4 atoms of carbon; but four times six is 24, not 22·22. If, therefore, transmutation by isomeric synthesis of carbon into silicon occur, it must, according to this view, be accompanied by a destruction of matter equal to the difference between 24 and 22·22; or, for every 24 parts by weight of carbon subjected to transmutation, only 22·22 of silicon would be obtained. I did not allow this difficulty to stand in the way of my repetition of the silicon experiments, as I saw a way of overcoming it. I shall mention what this was, without entering into any details in the way of vindication of its truth or probability. Let the received atomic weight of silicon, 22·22, be diminished by removal of the decimals, and made the round number 22. Such an alteration will, not improbably, be made by chemists, apart from all consideration of the question of transmutation. Then divide the received atom of carbon, 6, by 3, a liberty which would be conceded by many of my brethren, and it becomes 2; of which silicon is a multiple by the whole number 11. 11 atoms of carbon might, by synthetic transmutation, become 1 atom = 22 silicon, without any difficulty in the way of atomic weights.*

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* In a letter to the Lord Provost of Edinburgh on Dr Brown's claims to the Chair of Chemistry, which was printed and widely circulated, but not published.

From all that I have said, it will be manifest that no light task awaits those who propose to labour in the cause of transmutation. In the particular case of silicon, the question between Mr Knox and Dr Brown is one which can be settled only in the laboratory. It is possible that both of these gentlemen are right in their views. Nitrogen may be a compound of silicon and hydrogen, and silicon nevertheless, a compound or form of carbon. To do the subject justice, would require a careful repetition of all Dr Brown's and all Mr Knox's experiments, besides a lengthened series of independent researches, which would occupy at least six months of unremitting labour. The fact of an anomalous production of silicon is not beyond dispute; and till it is, the practical chemist cannot be expected even to consider the question of transmutation. Should the anomalous production of silicon, however, be fully confirmed, I think there are few who will not agree with me in wishing, that, whatever be the fate of Mr Knox's explanation, Dr Brown's theory should prove true. It seems absurd to wish that a law of nature should prove one thing rather than another; as if the law, when discovered, could be other than of God's making, and the best that can be. But what I mean is this:—Mr Knox's view, whilst it cuts off nitrogen from the list of simple bodies, reveals no general principle applicable to the reduction of the number of remaining elements. But if, with Dr Brown, we could effect the transmutation of one of these, sooner or later we should assuredly succeed in effecting the transmutation of all. If we can find a key, that will unlock in this way the intricacies of one group of elementary bodies, we may fully believe that the same instrument, or one fashioned like it, will open for us the mysteries of the rest.

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* I need scarcely say, that such a speculation possesses at present not the slightest value, and was pursued only at a time when I believed that there was full demonstration by experiment of the transmutability of carbon into silicon.

The recent researches of Dumas, Erdmann, and other continental chemists, have shewn, that the atomic weights of several of the elementary bodies (carbon, nitrogen, calcium, barium, strontium) are multiples, in whole numbers of that of hydrogen; and many, both in this country and abroad, encourage the expectation, that the equivalents of all the elements will prove, according to Dr Prout's hypothesis, multiples of hydrogen in the same way. I was willing to hope, that the atom of silicon at least, which, owing to the difficulty of procuring that substance, has been fixed on the evidence of comparatively few experiments, might prove to be a multiple of that of hydrogen by 22. This is a point to be decided solely by experiment.

As for the division of the equivalent of carbon by 3, it is acknowledged on all hands, that the received atomic weights may be multiples, or submultiples of the true ones. Thus, it is matter of dispute among chemists what are the true equivalents of copper, mercury, arsenic, phosphorus, antimony, and several others; and any alteration is hypothetically justifiable which does not contradict the law of multiple combination, and for which a sufficient necessity can be shewn. The justifying necessity in this case would have been the transmutation of carbon into silicon, and the acknowledgment of the atomic weight of the latter as 22.

In conclusion, it will be gathered from the brief and imperfect sketch I have offered, that the doctrine of Elemental Isomerism, and the transmutability of the elements, exists at present only as an unrealized idea, little, if at all, further advanced than it was in 1837, when first explicitly announced by Professor Johnston.

We are flung back, therefore, on the general analogies and probabilities that warrant the entertainment of such a doctrine; but these, I think, are neither slight in force nor scanty in number. All chemistry seems to me to point steadily and increasingly to the necessity of assuming, and, if possible, realizing such a law; and many of my brethren, I am certain, would agree with me in this. The scepticism so generally expressed as to the truth of Dr Brown's views, was directed rather against the processes and experiments by which he professed to establish his doctrine, than against the doctrine itself; and so far as this implied a resolution to accept nothing but the most rigidly quantitative experiments, in proof of so revolutionary a proposition as that of transmutation, it was quite justifiable. The instrument *par excellence* of chemistry is the Balance; and every chemist must expect to have his discoveries literally and metaphorically weighed in it, and rejected if found wanting. The Familiar Letters of Liebig, *e.g.*, show that, although he unhesitatingly and too summarily condemns Dr Brown's experiments, he willingly speculates on the light which Isomerism may throw on the true constitution of the elements.

And if chemistry is in favour of the doctrine we are considering, the other physical sciences justify it also. The geologist acknowledges the existence of many phenomena, in the relative distribution of the materials forming the earth's crust, which seem inexplicable by our present chemistry. The naturalist affirms that the whole subject of fossil zoology is plunged in mystery; and anxiously demands if the appearance of substances in fossils, which no one can trace to ordinary sources, does not depend upon a transmutation of some of the pre-existing ingredients of these bodies.* The agriculturist is frequently perplexed, in his endeavours to trace the constituents of the plants he cultivates to the soil they have grown upon. The difficulty is generally got over by the accusation of imperfect analysis; but some have courage enough to refuse this *argumentum ad ignaviam aut ignorantiam*, and one, Mr Rigg, who has been studying the subject for years, declares that his observations have led him to the conclusion, "that of the elements, carbon, hydrogen, oxygen, nitrogen, sodium, potassium, calcium, &c., which constitute the organic and inorganic parts of plants, hydrogen is the only ultimate element, the rest being all compound bodies; and to question the compound nature of hydrogen."*

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* I refer particularly to a discussion which took place last winter in the Zoological Society of London, as to the source of the fluoride of calcium which appears in fossil bones. Literary Gazette, 2d December 1843, p. 773. The subject was afterwards referred to by Mr E. Solly, in a lecture at the Royal Institution.

Encouraged by these things, I, for one, will, in faith and patience, abide the issue, ready and willing, should I again see as much encouragement as I did last autumn, to spend another winter, or many winters, in endeavouring to bring about a consummation so devoutly to be wished, as the manifestation of the essential simplicity and unity of matter.

On the Volume of the Niagara River, as deduced from Measurements made in 1841. By Mr E. R. BLACKWELL; and calculated by Z. ALLEN.

Very little attention appears to have been hitherto bestowed on the investigation of the comparative volumes of water discharged by the great rivers of the globe. The relative amount of the evaporation and drainage from the soils of different countries, in proportion to the quantity of rain that falls upon each, as denoted by rain-gauges, is also another interesting subject connected with the preceding one; for by measuring the quantity of water discharged from a region of country by the streams that drain it, and by deducting this quantity from the whole amount that falls upon it, as indicated by rain-gauges, the relative amount of evaporation may be ascertained. The investigation of these facts forms the basis of a branch of the science of hydrography, and leads to many useful as well as curious and interesting inquiries.

Whilst passing a few days at the Falls of Niagara, in the summer of 1841, it occurred to me to make the necessary admeasurements for ascertaining the quantity of water precipitated by the grand cataract, and drained from the vast area of country bordering on the great lakes of North America. This subject has long remained a mere matter of conjecture, although unusual facilities are offered for making the admeasurement of the volume of this majestic river, from the circumstance of its issuing from Lake Erie, in an average equalised current throughout the various seasons of the year, unaffected by the droughts of summer and the floods of winter. In order to ascertain the average volume of water discharged by most other rivers of the earth, it becomes necessary to multiply a great number of observations during the several seasons of the year. But the flow of the Niagara River remains always nearly the same, varying only from the action of winds on the surface of Lake Erie, and from a periodical succession of several rainy or dry years in the broad regions of the upper lakes.*

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* Experimental Researches, &c., shewing Carbon to be a compound body made by Plants. By Robert Rigg, F.R.S. p. 264.

The results of the admeasurements of the volume of water of Niagara River, are now submitted, with the hope that they may furnish facts in this branch of the science of hydrography which will be used as data by scientific men for various calculations; and with the hope, also, that others may be induced to commence a system of similar admeasurements of the other great rivers of the earth, such as the Mississippi, Ganges, &c., which may form a basis of comparison of their relative magnitudes.

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* It appears from the best information I was enabled to obtain, that a strong breeze or gale on Lake Erie, in the direction of the outlet of this lake, will cause the waters to become heaped up at that end, so as to produce a rise of the level of about two feet, and a corresponding rise of the Niagara River. A subsidence of the level of the surface to an equal extent occurs, whenever a gale takes placed in an opposite direction, making a total variation of about four feet in the rise and fall of the level of the river, from the simple action of the wind on the surface of the lake. These changes of level have sometimes taken place in the course of a few hours. A nearly equal, but more gradual change of level is produced, as before stated, by the alternations of a period of several rainy years followed by a period of successive years of comparative drought. The descent of the waters of Niagara River, from the outlet of Lake Erie, is at first so considerable, as to cause the flow of the current to become accelerated to a velocity of about eight miles per hour. By means of an embankment, constructed parallel with the shore, along the margin of the river, the level of the surface of Lake Erie is maintained or upheld, through a distance of several miles, above the level of the descending stream. This embankment serves to form a portion of the Erie Canal, and also to convey a supply of water to several large flour-mills at Black Rock; thus affording an efficient fall of about five feet.

From the general levelness of the low banks of the river between Black Rock and Lewiston, it appears probable that water-power, to any extent that ever will be required, may be obtained by diverting the water of the Niagara River over the table land adjacent to its bed; and that mills might there be erected sufficient to grind all the wheat produced on the broad regions of country whose tributary waters swell the great lakes, and the Niagara affording unrivalled facilities for transporting the wheat to these mills.

I have also subjoined some calculations, from which it will appear that the motive-power of the cataract of Niagara exceeds, by nearly fortyfold, all the mechanical force of water and steam power rendered available, in Great Britain, for the purpose of imparting motion to the machinery that suffices to perform the manufacturing labours for a large portion of the inhabitants of the world, including also the power applied for transporting these products by steam-boats and steam-cars, and their steam-ships of war, to the remotest seas. Indeed it appears probable, that the law of gravity, as established by the Creator, puts forth in this single waterfall more intense and effective energy, than is necessary to move all the artificial machinery of the habitable globe.

In order that confidence may be placed in the estimates now presented, it may be proper to subjoin a statement of the modes in which the admeasurements were made, and the calculations based upon them were accomplished.

After having personally, and with much labour, rounded the fearfully rapid current of the Niagara river above the Falls, at Black Rock, where the bottom or bed appears to be nearly level from one side to the other, and the depth about thirty-two feet; and having repeated a course of similar admeasurements below the falls at Queenston, where the current is more placid, and the depth in the deepest place about one hundred and sixty feet; and after having lost an anchor in the course of these experiments, I finally found it necessary to have recourse to the aid of an engineer, in order to perfect all the admeasurements, which my limited time would not allow me to complete. For this purpose the services of Mr E. R. Blackwell, of Black Rock, a most skilful and accurate engineer, were engaged by me. His residence at that time in the immediate vicinity of Black Rock, enabled him, with his zeal for the accomplishment of this object, to devote much time to completing an exact survey. By reference to Mr Blackwall's elegant map of a section of Niagara River opposite Black Rock, it will be observed that thirty-eight soundings were taken in three distinct ranges or lines across the channel of the river, each of the ranges being at the distance of six hundred and sixty feet apart.* After thus obtaining three cross sections of the volume of the current, whereby its area or dimensions were ascertained, the velocity of the surface was then found in ten different places between these three lines, by noting the time in which floating bodies set adrift in different parts of the width of the river, were borne down from one sectional line below it. All these admeasurements were made with every attention to accuracy.

Having thus found by experiment the velocity of the surface of the stream, the average or mean velocity of the bottom and middle, as well as of the surface, was ascertained by means of the formula established by Eytelwein ($v = \frac{v}{10} \times 9$), which for measuring the volume of water flowing in rivers of great depth, I consider to be a closer approximation to accuracy than those established by Prony and other philosophers, who have investigated the subject of the discharge of water flowing down the inclined planes of the beds of rivers. These calculations have been carefully revised; and the results stated may, therefore, be deemed as a sufficiently accurate estimate of the volume of water that flows down from Lake Erie.

Allowing about 374,000 cubic feet of water (by estimate) to flow through the harbour of Black Rock per second, as indicated on the map, the results of their calculations shew that about 22,440,000 cubic feet, or 167,862,420 gallons, weighing 710,250 tons, or 1,402,500,000 lbs. of water flow out of Lake

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* In Mr Blackwall's map the left hand column of figures in each section represents the distance of each sounding from the American shore, and the right hand column the depth of the soundings. The distance of the soundings from each other may be found by subtracting each measurement from the one next above it. The arrow denotes both the direction of the current and the point of compass.

Erie every minute, and become precipitated over the cliffs of rocks at the grand cataract of the Falls of Niagara.

Estimating the perpendicular descent of the waters of the grand cataract to be one hundred and sixty feet, and making the usual allowance of one-third part for waste of effective power, in the practical application of water to water-wheels; and also estimating the power of a horse to be equal to a force that will raise a weight of 33,000 pounds one foot high in one minute, which is Watt and Bolton's standard, we obtain the following results:

$$\left.\frac{1{,}402{,}500{,}000 \text{ lbs of water} \times 160 \text{ feet of descent}}{33{,}000}\right\} - \tfrac{1}{3}$$

= 4,533,334 horse-power, which is the measure of the mechanical force, or motive power, that the waterfall of Niagara is capable of imparting.

It has been estimated by Mr Baines, in his History of the Cotton Manufactures of the United Kingdom of Great Britain in 1835, that the motive-power employed to operate the machinery of all the cotton-mills in Great Britain was then equal to that of about

33,000 horses, imparted by the agency of steam.
11,000 ... ... waterfalls.
100,000 horse-power he estimated to be employed to operate the woollen, flax, and other mills, and mechanical operations.
50,000 horse-power for propelling the machinery of steam-boats and coal-mines.

———

194,000 horse-power in the year 1835.

Supposing about 20 per cent. to have been added to this motive-power in the increase of locomotive engines for railways and steam-boats, as well as for various manufacturing purposes, since
39,000 1835, we add to this aggregate 39,000 horse-power
——— more,

233,000 horse-power may be taken to be the aggregate of motive-power of all the steam-engines and improved waterfalls of Great Britain; which, it will be perceived, is only 1-19th part of the effective water-power of Niagara Falls.

When it is considered that the water-power of the cataract of Niagara is unceasing, by night as well as by day, and that the power, as calculated above, for practical purposes in Great Britain, is only applied, on an average, about eleven hours per day, during six days of the week, it may be assumed that the motive-power of Niagara Falls is at least forty-fold of the aggregate of all the water and steam power employed in Great Britain, and probably equal to the aggregate of all the motive power employed for mechanical purposes on this earth.

The surface of Lake Erie is found to be 331 feet above the surface of Lake Ontario, and 565 feet above that of the ocean. The descent of the waters of Niagara River, in the few miles of distance between Black Rock and Queenston, is about 171 feet, exclusive of the grand cataract itself, forming a succession of rapids, which, in some places, present to view the sublime spectacle of the agitated surface of the ocean in a storm; and these rapids continue to occur during the subsequent descent of the river St Lawrence, from the level of Lake Ontario to that of the sea, making, in the aggregate, above three-fold of the waterfall of the grand cataract, and, consequently, one hundred and twenty-fold of all the physical power derived from the use of all the waterfalls and steam-engines employed, as above stated, in Great Britain, omitting to take into account the several huge rivers that are tributaries of the St Lawrence. Such, and in so great a scale, are the ordinary operations of the impulses of physical power employed in the "mechanics of nature," in governing the movements of the waters of a single river, exceeding manyfold the portion of physical forces rendered available and employed by all the inhabitants of the earth, as a motive-power in the "mechanics of the arts." There is thus furnished an impressive lesson to humble the pride of man in his boasted achievements of the triumphs of mind over inert matter. It is well that these considerations should occur to the spectator, whilst he regards the cataract of Niagara; for nowhere is there exhibited on this earth a more impressive spectacle of the display of energetic physical power. Cold and indifferent, indeed, to the highest attribute of Omnipotent excellence, must be the mind of that human being, who can raise his eyes from the contemplation of this sublime work of nature, without a glow of fervent admiration of the " might, majesty, and power " of nature's God.—*The American Journal of Science and Arts*, vol. xlvi. No. 1. January 1844, p. 67.

On a Carbonaceous Deposit, or Film, on the Lakes of Westmoreland. By JOHN DAVY, M.D.,F.R.S., L. and E. Communicated by the Author.

Although the lakes of Westmoreland, in common with its streams, are distinguished for the purity of their waters, yet occasionally their surface may be seen covered to some extent with a blackish film. I have observed such a film, or pellicle, drifted, as it seemed, by the wind, when very gentle, not only on Windermere, and the Rydal and Grasmere lakes, but also on Easdale tarn, a small secluded mountain lake.

The matter of this pellicle I have found, on examination, to be chiefly carbonaceous, and very like the matter of soot from coal: thus, it has deflagrated when heated with chlorate of potash; it has taken fire and consumed without flame before the blow-pipe; triturated with water, so as to be thoroughly wetted, it has sunk; and, under the microscope, it has appeared to consist of particles of irregular form, varying in size from $\frac{1}{4000}$ inch in diameter, or under that, to $\frac{1}{1000}$ of an inch, or more; and transmitting, like particles of soot, a brownish light.

Having the properties of soot, must it not be inferred that the matter of this film is soot in reality, wafted probably from a distance, from the great manufacturing districts of Lancashire and Yorkshire, and brought down, when floating in the atmosphere, by rain; the mountains of the lake country acting as great refrigeratories, tending to arrest and condense, or precipitate, the vapours rising from lower and warmer levels? What is in favour of this view is, that the pellicle is most commonly seen after heavy rains, succeeded by a calm or a gentle breeze, and also that a similar matter is sometimes observed on the surface of snow on the remote hills and mountains.

When it is considered to what vast distances the sand of the desert, and volcanic sand, and the spray of the sea, have been carried by the wind, it is easy to conceive that soot, the matter of smoke, may be wafted through the atmosphere, even farther than is required, on the supposition that the main source of the film in question is the adjoining manufacturing districts.

Liebig has detected in the rain water of Germany, besides carbonate of ammonia, fecal matter, that is, the odour of this matter. I have examined many samples of rain water collected at Ambleside, and have always detected in them ammonia, but never the offensive smell alluded to. I have found, too, it may be remarked, collected in the funnel of my rain-gauge, to the terminal pipe of which a piece of linen is fastened as a filter, a notable portion of soot, exactly resembling the matter of the pellicle of the lakes. But, though I believe that the greater part of it was brought from a distance, I would not insist on this, inasmuch as it may be said to have been derived from the smoke of the adjoining village.

I have made no mention of the quantity of black matter observed on the Westmoreland lakes, or on its duration. Its duration is generally short, disappearing after disturbance of the water by the first heavy wind, when being wetted by the agitation, it is probably diffused through the water, and shortly sinks. Its quantity is often considerable, quite precluding the idea of its being derived from the villages and hamlets of the country; and is of such common occurrence, especially on Windermere, that some gentlemen keeping pleasure-boats, from its blackening effect, have ceased, I am informed, to paint the bottoms of them white.

The Windermere boatmen, with whom the appearance is familiar, imagine that the black matter rises from the bottom of the lake; a supposition which can hardly be maintained, being incompatible with the specific gravity of the substance when wet, when, as I have already observed, the matter of the film sinks in water. Another supposition regarding its origin, which at first view seems more probable, is, I believe, equally untenable, viz., that the black matter is of the nature of peat, and is washed down from the hills from beds of peat. The objections to this are, that the microscopic character of the substance of the film is different from that of peat, being without vegetable fibres; and that the water of the lakes and mountain tarns of Westmoreland are never, that I am aware, discoloured by peat, which, indeed, is not abundant in the lake district, is chiefly confined to the hollows of the higher hills, and, I believe contains very little soluble matter capable of imparting a brown colour to water, supposing that this colour is owing to dissolved mater, and not merely to suspended particles of peat.

Whether a film of soot, or of black matter, such as I have pointed out, has been observed on any other lakes, I am ignorant; I am not aware that it has been described before. Were attention, however, paid to the subject, it is probable that the same appearance would be observed elsewhere; and it may be deserving of attention, not only as a matter of curiosity, but also as an indication of currents of air, and of the course and spread of effluvia.*

THE OAKS, AMBLESIDE, 7th May 1844.

Outlines of Mr W. Hopkin's Researches in Physical Geology. 1st, 2d, and 3d Series. (London, 1839–1842.) By CHARLES MACLAREN, Esq., F.R.S.E. Communicated by the Author.

Physical geology, which treats of the structure of the globe in mass, without reference to the succession of rocks at the surface, has been greatly advanced by the labours of this able mathematician. The results to which the present researches have conducted him are new and curious, as well as geologically important. So far as I know, they have not yet found their way into any of the geological works now in circulation; and for this reason, as well as on account of their intrinsic value, a popular outline of these results may be useful.

Modern science is rich in wonders. Who would think that the sun and the moon, bodies so distant, and of which in most respects we know so little, could be interrogated respecting the structure of our globe in those deep recesses to which we, who live on its surface, cannot find access—ay, and should be able to return us distinct and instructive responses? Such is the case, as Mr Hopkins has shewn in these very learned papers.

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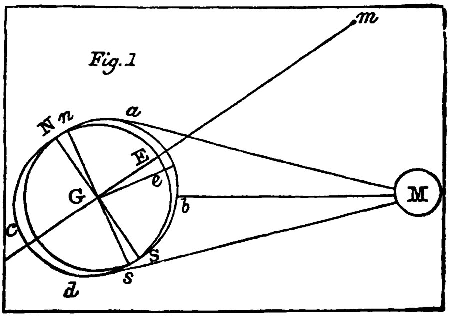
* The black-rains of Canada, also the film of carbonaceous matter observed on the surface of Loch Earn by Mr Milne, as noticed in one of our volumes, appear to have the same origin as that above described.—EDIT.

Mr Hopkins's conclusions rest entirely on the effects of the sun and the moon's attraction, as indicated by the phenomena of Precession and Nutation. Any analysis of his elaborate calculations would be out of place here. What I propose, is to give a general idea of the basis and form of his argument, and an abstract of his conclusions.

It is known that the sun itself, and all the planets and secondaries of the solar system, attract each other with forces directly proportioned to their masses (that is their weights), and inversely proportioned to the squares of their relative distances. It is known also that the earth is not a perfect sphere, but an oblate or flattened spheroid, the equatorial diameter of which exceeds the polar by 1-300th part. To express it in another form, the equatorial parts are thirteen miles farther from the centre than the polar.]



In the above diagram, the round figure G represents a section of the globe through its axis, N the north and S the south pole; *a b* the equatorial protuberance on one side of the globe, *c d* that on the other. If the earth were a perfect sphere, the moon's attraction would have no disturbing effect upon it. And though it is an oblate spheroid, the result would be the same if the moon's orbit were in the plane of the equator, that is in the position G *m*, as in this case the force of attraction on the one side of the earth would exactly balance that on the other. But the plane of the moon's orbit is oblique to that of the equator, or in the line *b* M; and as the distance between the satellite and the earth is only thirty diameters of the latter, the action of the moon M is a little greater on *a b*, the part of the protuberance next to it, than on the part opposite, *c d*. The effect of this disturbing action is to draw down the plane of the equator from the direction G E to the direction G *e*, and to produce a corresponding angular change in the position of the earth's axis, shifting it from N S to *n s*. This change of position is called the *Nutation of the earth's axis* (from *nutatio*, nodding). The name is appropriate, for the motion is constantly varying in amount, and constitutes a sort of tremor or vibration, which runs through its principal phases in eighteen and a-half years, the period in which the moon's Nodes complete their revolution. The action of the sun is conjoined with that of the moon, but is comparatively feeble. The secondary effect of this Nutation is the *precession of the equinoxes*, or the shifting of the equinoctial points 50′ westward annually, which makes the pole of the earth describe a circle of 47° in diameter round the pole of the ecliptic in 25,800 years.

The thickness of the equatorial protuberance *a b c d*, and the magnitude of the angular change in the earth's axis N *n*, must not be judged of from the figure, in which they are necessarily exaggerated. The equatorial protuberance amounting only to 13 miles upon a semi-diameter of 4000, may be compared to a band of writing-paper wrapped round the middle of an orange. The nutation makes the pole N describe a very small circle round its mean place, namely, of about 900 feet radius. To give an idea of the extreme minuteness of the change, let us suppose an iron rod 100 feet long, fixed at one end and moveable at the other, to represent one-half of the earth's axis. If the moveable end were pulled *the twentieth part of an inch* to one side, the deviation would be proportionally as great as that which the *lunar* nutation produces in the terrestrial axis.

The earth's equatorial diameter exceeds, I have said, the polar only by a 300th part. The moon's attraction, therefore, may be considered as acting upon the part *a*, with the aid of a lever, a fraction longer than if the earth had been a perfect sphere. The difference is very small; but when we also recollect that the moon's attraction at *d* counteracts her attraction at *a*, and that it is only the difference between the one attraction and the other, depending on the inequality of the distance, which disturbs the earth's position—and further, that the mass or weight of the disturbing agent, the moon (which is the measure of her power), is only the 68th part of that of the earth—when all these circumstances are considered, it might be inferred that the effect of causes so very minute would be inappreciable. Such an inference, however, would be erroneous. In truth, the *effect* was discovered first, and led to the knowledge of the *cause*. Dr Bradley detected a change in the latitude of the stars, which, after increasing for nine years, diminished for the next nine, and amounted in all to eighteen seconds. He observed that its period coincided exactly with that of the revolution of the moon's nodes, and was thus led to the discovery of the cause.

Cavendish's celebrated experiments with lead balls have been lately repeated at the expense of Government; and the conclusion drawn from them is, that the mean density or weight of the earth is rather more than 5½ times the weight of an equal bulk of water.* Now, the rocks at the surface are only about 2½ times the weight of water, and to make up the mean density or weight of the whole to 5½, it follows that the interior must be as much above that as the surface is below it. We thus arrive at the conclusion, that the density increases with the depth beneath the surface.

Astronomers simplify the problem by considering the protuberance, *a b c d*, as a ring detached from the spherical mass. The action of the moon in shifting the axis of such a ring, revolving in free space, would be very great; but it is reduced to the very minute quantity I have mentioned, because the sphere, to which the ring is attached, has no tendency to change its position, and resists the change in the ring by its *inertia*. The ring, in short, has an incomparably larger mass to drag after it, and hence undergoes but a very slight change of position.

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* The proportion, as given in the last volume of the "Penny Cyclopædia," is 566 to 100. This work, though bearing a humble title, is invaluable for the accuracy and great amount, as well as the accessible form, of the knowledge it communicates.

The magnitude and density of the globe being known approximately, and also the magnitude and density of the ring *a b c d*, the action of a body like the moon, whose mass and distance are known, can be subjected to mathematical calculation.

Mr Hopkins first investigates the phenomena of precession and nutation on the hypothesis that the earth is of uniform density throughout; and in this case the conditions of solidity and fluidity are considered. His chief object here seems to have been to test the accuracy of his process; and we need not, therefore, stop to notice the results. He then passes to the case in which the earth is assumed to be, what it really is, a body whose density is variable, increasing with the depth, and is modified by the conditions of internal fluidity and solidity.

The mean density of the globe is about the same with that of the heaviest iron ore, or 5½. Keeping in mind that the force of attraction is in proportion to the quantity of matter, let us assume the ring *a b c d* to be of the same density with the sphere G, or five and a half times the weight of water. In this case its effect, under the moon's attraction, in disturbing the position of the earth's axis, would be in proportion to its relative mass, and its distance from the earth's centre. But if the density of the ring were only that of brick, or two times the weight of water, its disturbing effect upon a sphere of the superior density of iron ore would be comparatively trifling. If, again, the ring had merely the density of pine wood, which is but the eighth part of that of iron ore, its disturbing effect would scarcely be appreciable by the nicest observation.

But the density of the ring, and the average density of the globe, are not the only elements involved in the problem. We have further to inquire into the constitution and distribution of the matter in the interior of the globe. We know that it is solid at the surface. It is solid to the centre—that is to say, is it composed of parts immoveable *inter se?* We know also from volcanoes, that there is fluid matter within it, that is, matter whose parts are moveable, and obedient *inter se* to the laws of gravitation, external attraction, and centrifugal force. Does this fluid matter compose a large or a small part of the entire mass? Is it situated near the surface, or at a vast depth? The disturbing action of the moon will not be the same upon a globe all solid, and upon one nearly all fluid; will not be the same upon a globe in which the solid shell forms one-half of the mass, and another in which it forms only one-tenth.

These statements will convey a general idea of the conditions of the problem which Mr Hopkins had to solve; and he seems to have been careful to examine it under all its various forms.

The conclusion to which his researches have conducted him is thus announced:—"*Upon the whole, then, we may venture to assert, that the minimum thickness of the crust of the globe, which can be deemed consistent with the observed amount of precession, cannot be less than one-fourth or one-fifth of the earth's radius.*" That is from 800 to 1000 miles.

Let it be observed that this is the *minimum* thickness consistent with the known precession. The actual thickness may be much greater. The globe may even be solid to the centre, and this, too, without very materially altering the conditions of the problem; for if the shell is 1000 miles thick, it constitutes *four-sevenths* of the bulk of the globe; and though the remaining *three-sevenths* may have a higher density, the action of disturbing forces upon them from without is greatly lessened by their central position. Two other elements are yet wanting to give us complete information on the points in question. These are the effect of pressure in producing condensation in the matter of the globe, and the effect of heat in resisting it. Professor Leslie made some experiments to ascertain the relation of density to pressure, from which he inferred, that at the depth of 400 miles, or one-tenth of the semi-diameter, marble would have its density increased nearly one-half, while water would have its density more than quadrupled, and would, in fact, be heavier than marble. He hence concluded, that the density of the globe must increase so rapidly in the interior, that if it consisted either of solid or fused matter to the centre, the mean density would greatly exceed five and a half times the weight of water; and, therefore, that it must consist of a hollow shell, the cavity of which is probably filled with some extremely elastic substance, such as elemental fire or light. In these calculations, the modifying effects of heat were left out of view ; and partly, perhaps, on this account, partly from the insufficiency of the data, no great importance seems to have been attached to the result by men of science. So far as I can see, Mr Hopkins's conclusions would only be partially affected by a more accurate determination of the relations between pressure, density, fusibility, and heat. Such determination would merely add a little to, or subtract a little from, the thickness of the crust, which must still remain a large submultiple of the radius.

From the rapid increase of heat as we descend beneath the surface, it had been inferred that a temperature sufficient to fuse every known substance, would be found at a very moderate depth. Cordier, one of the first who took a comprehensive and scientific view of the subject, thought that the thickness of the crust did not probably exceed sixty miles, but might be much less ; and he further inferred that the lavas flowing from volcanoes were merely portions of the great central reservoir of molten matter, squeezed out in consequence of a slow contraction of the shell of the globe, produced by secular refrigeration.* Like Mr Leslie, he made no allowance for the antagonism betwixt heat and pressure. It was justly objected, too, that the supposition of the depth of the solid envelope being so small—only one-sixty-sixth part of the semi-diameter —in comparison with that of the fluid mass within, was inconsistent with the known stability of the earth's surface.

Mr Hopkins's conclusion, no doubt, rests on a narrow enough basis. It is something like an estimate of the distance of the stars, deduced from a difference of one or two seconds in their apparent position—a difference scarcely distinguishable from errors of observation—but in the absence of more direct and positive evidence, we are thankful to obtain it. I believe it to be correct in principle, and that its errors, if any, are errors of degree ; and the views respecting the structure of the globe, to which it conducts us, are in harmony with facts derived from other sources.

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* Essai sur la Temperature de l'Interieur de la Terre ; lu à l'Academie des Sciences, Juin 1827. His estimate is 20 leagues of 5000 metres.

"The results arrived at," says Mr Hopkins, "have an important bearing on our physical theories of volcanic forces, and the mode in which they act. Many speculations respecting actual volcanoes have rested on the hypothesis of a direct communication, by means of the volcanic vent, between the surface and the fluid nucleus beneath, assuming the fluidity to commence at a depth little, if at all, greater than that at which the temperature would suffice, under merely the atmospheric pressure, to fuse the matter of the earth's crust. When it is proved, however, that that crust must be several hundred miles in thickness, the hypothesis of this direct communication is placed much too far beyond the bounds of probability, to be for an instant admitted as the basis of theoretical speculations. We are necessarily led, therefore, to the conclusion that the fluid matter of actual volcanoes exists in subterranean reservoirs of limited extent, forming subterranean lakes, and not a subterranean ocean. Such, also, we conclude, from the present thickness of the earth's crust, must have been the case for enormous periods of time; and, consequently, there is a very high degree of probability that the same was true at the epochs of all the great elevations which we recognise, with the exception, perhaps, of the earliest."

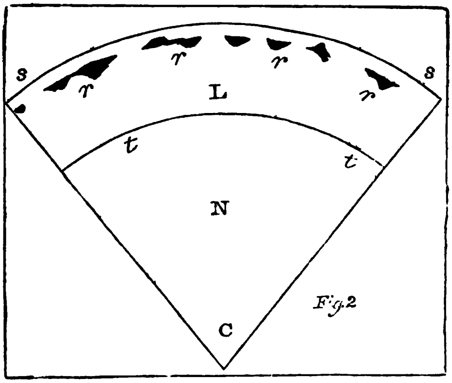

Fig. 2

Let figure 2 represent a section of a portion of the globe to illustrate the hypothesis.

C The centre of the globe.

L The shell or crust of solid matter, whose depth is assumed to be 1000 miles, or one-fourth of the radius *s* C.

s s The surface of the globe, or outer boundary of the crust.

t t The inner surface or boundary of the solid crust.

N The interior, which may be filled with matter, either fluid or solid.

r r Reservoirs of fluid matter, at a moderate depth under the surface, which produce movements of elevation and give birth to volcanoes.

Mr Hopkins thinks the origin of these subterranean lakes or insulated masses of fused matter, may be ascribed to two causes; first, the greater fusibility of the matter composing them; and, secondly, a relaxation of the pressure, which counteracts fluidity. We know that there is a great difference in the fusibility of the rocks forming the outer parts of the globe; and we have no reason to doubt that there may be a similar difference in the matter existing at a greater depth. We know, too, that when certain substances which act as fluxes happen to be present, fusion is facilitated. The greater fusibility of some parts being admitted, Mr Hopkins shews how it might be sustained or increased by upheavals; and his hypothesis has the further merit of explaining very happily a phenomenon attending elevatory movements which has hitherto puzzled geologists.

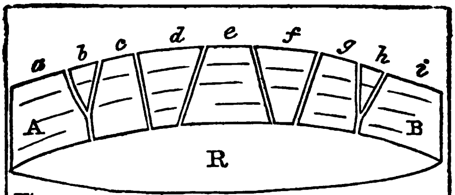

Fig. 3

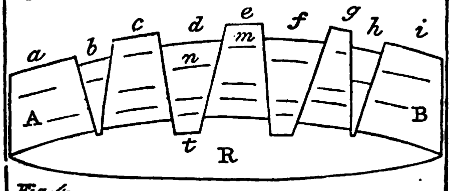

Fig. 4

Let A B, fig. 3, represent a transverse section of a portion of the outer crust of the globe, some miles thick, which has undergone an elevatory movement, and been fissured by it.

R The cavity containing the fluid matter, whose expansion or intumescence has heaved up A B. "The fissures," says Mr Hopkins, "will scarcely ever be exactly parallel, and therefore will meet if sufficiently produced." The diagram shews them before they have undergone any displacement. Some of the separate masses are complete wedges, as *b* and *h*; some truncated wedges with their broad sides upwards, as *d f*; and some truncated wedges with their broad sides downwards, as *a c e g i*. The formation of these fissures will be completed at nearly the same instant of time. Conceive the mass A B to be then further uplifted. The fissures will not be farther widened; for the complete wedges *b h*, which do not reach down to the fluid mass below, will descend by their gravity into the position shewn in fig. 4. The truncated wedges also, *d f*, having their narrow sides downwards, will encounter less resistance from the fluid mass below than *a c e g i*, which have their broad sides downwards, and will also descend; and thus the different masses will arrange themselves as shewn in fig 4, forming an arch *which will sustain itself*. Supposing, then, the cause of the intumescence of the fluid to cease acting, and that fluid to return to its original dimensions, the pressure of the superincumbent mass A B may thus be wholly or partially removed from the fluid. "Hence, assuming that solidification is promoted by great pressure, it evidently appears how a portion of the interior mass might be maintained in a state of fluidity by the removal of a superincumbent pressure, which would otherwise have brought it to a state of solidity."

"It is not essential to assume that the arch shall entirely support itself. It may be partly supported by the fluid beneath, or it may break down in certain points, or along certain lines, and form there new supports intermediate to the extreme ones. Instead of one continuous internal lake, a number may thus be formed, connected with each other by more or less obstructed channels of communication."

The phenomenon previously mentioned, which this hypothesis so well explains, is the following:—When *faults*, or shiftings of the strata, occur in mines, it is always found that the dislocated portion is to be sought above or below, according as the line of fault, traced downwards across the bed, inclines outwards from, or inwards to, its plane. Thus if *m*, a bed in the mass *e*, is cut off by the fault *t* dividing *e* from *d*, the miner seeks for the prolongation of it downward, and will find it at *n*, because the line of fault *t* inclines outward from the plane of the bed. If, again, he had been working on *n*, he would have sought the prolongation upward, because the angle inclines in the opposite way. The explanation is, that the fluid matter in the cavity R exerts a greater pressure on the masses *a c e g i*, whose broad sides are downwards, than on *b d f h*, whose narrow sides are downwards, and in the general movement the former are therefore pushed farther up than the latter. Or, to express it in another form, the latter slip down till their immersion in the fluid counterbalances the narrower surface exposed to its pressure. This curious fact in mining has long been observed; and, so far as I know, it has hitherto baffled the ingenuity of geologists to give even a plausible explanation of it.

The hypothesis of the fluid matter existing in isolated cavities at a moderate depth, enables us to explain some of the phenomena of volcanoes by the operation of an agent which is known to be always present, namely steam or watery vapour. Professor Bischof of Bonn, in an elaborate and learned memoir,* calculates that steam, at its maximum elasticity, is capable of supporting a column of liquid lava 17 miles in height. The depth at which the internal heat of the globe would suffice to keep lava in a state of fusion, is estimated at 20 or 30 miles; but the data are too imperfect to indicate the ratio of increase with any certainty. The increase, too, may follow a geometrical, instead of the arithmetical, ratio assumed; and in this case the depth will be much less. Besides, if there is an excess of heat at the bottom of a reservoir, the matter may be kept fluid by circulation, to a level much nearer the surface than the supposed limit of fusing temperature. Watery vapour, also, may not only reach the cavity containing the fluid matter, but may mingle with it through chinks in the volcanic vent; and, "as a bubble of air let into a barometer tube drives the mercury into the Torricellian vacuum far above the barometric height, aqueous vapour may raise a column of lava of a height equal to its expansive force into the channels opening into the craters."* We may thus have a continual alternation of columns of lava and steam rising in succession, the consequence of which would be an alternate ejection of lava, red-hot masses, and clouds of vapour, as well exemplified at Stromboli.

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* Edinburgh New Philosophical Journal, January and April 1839.

Now, to enable the steam to exert the prodigious force required, we must suppose both it and the lava on which it acts to be lodged in a cavity, surrounded on all sides by solid resisting walls, with no opening but the volcanic vent and the chinks by which water is admitted. If lava were part of a central fluid nucleus, water reaching it at any point, after being converted into vapour, would glide along the under part of the solid crust, and settle at the highest vaulted cavities, till additions to its quantity or its temperature enabled it to open a passage for itself, or for a portion of the lava, through the crust. Water passing downwards from the ocean near the shore, might thus create a volcano at the distance of 500 or 1000 miles inland, as readily as near the coast. But the fact of all active volcanoes being near the sea, or large bodies of water, is at variance with this supposition. On the contrary, it lends support to the conclusion, first, that water is a necessary agent in volcanoes; and, next, that the fluid matter upon which it acts exists in isolated basins of various forms and dimensions, confined within solid rocks. Thus, one basin 150 miles in length may exist under southern Italy, connecting Vesuvius, the Lipari Isles, and Etna. There may be one 200 miles in length and breadth, under Iceland; and a vast trough 4000 miles or more in length, but of comparatively small breadth, may extend under the Andes. The sudden and simultaneous activity of three volcanoes in the Cordillera, far distant from each other, which broke out from a state of repose into violent eruption on the same day, favours the idea of a subterranean connection between them; and the existence of an almost continued line of craters in the intermediate spaces, strengthens the probability of such a connection. The three volcanoes were, Osorno in lat. 40° S., Concagua in 32° S., and Coseguina in lat. 13° N., and the day of their simultaneous eruption was 20th Jan. 1835.—(Mr Darwin, Trans. Geol. Soc., March 1838.) If the volcanoes at the two extremities of the line sympathize in their action, we would expect all the intermediate parts to be disturbed less or more; and the extreme frequency of earthquakes in the Andes (often several in a day) may thus be the natural consequence of the long series of craters, the activity of any one of which will agitate the whole chain in a less or greater degree.

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* Bischof's Paper, p. 38.

The hypothesis helps us to explain local elevation and local subsidence. Fused matter must be subject to greater changes of temperature than solid matter. The one is influenced by conduction and circulation, the other by conduction only. A small contraction or expansion in a fluid mass 5 miles or 50 miles deep, is sufficient alone to account for a portion of the external crust which forms its roof being raised or depressed, without calling in the agency of water at all. Then as to the causes of contraction and expansion, the fluid mass, independently of mere variations of temperature, may augment its bulk by dissolving a part of its walls, or diminish its bulk by partial or entire solidification. Again, let us suppose a large volume of steam to be permanently in contact with the fused mass, and so placed that it cannot escape, it will act like the air in the air-chamber of a fire-engine, and a small addition to its volume will force upwards, and eject, a part of the fused matter. I think, then, that the hypothesis of partial deposits of fluid matter has material advantages over that of a fluid nucleus. Firstly, it allows us to assign a thickness to the crust of the globe which is more in accordance with its known stability. Secondly, it offers greater probabilities of such changes taking place in the condition of the matter itself, as the phenomena require us to suppose. Thirdly, by placing each fluid mass within solid walls, it better explains how these changes may manifest themselves at the surface; because it allows us to suppose that the fused matter, confined and compressed within its rocky envelope, may ascend, on the same principle as the mercury ascends in the thermometrical tube. Fourthly, limited and local deposits of fused matter best account for limited and local movements—for certain portions of the earth's surface being repeatedly ruptured or disturbed, while others appear to be in a state of complete repose.

While it was supposed that the source of subterranean action was at a vast depth under the surface, it was natural to infer that the effects of one movement might extend over a vast space. In common with many others, I was accustomed to consider it as probable that earthquake shocks which synchronised within a few hours, though happening in distant countries, might proceed from one and the same effort of the plutonic force. This was merely a conjecture, favoured by some facts, and opposed by others. Additional light has lately been thrown on the subject by the researches of Mr David Milne. The register of the shocks at Comrie for some years past which he has published, and the record which he has collected of those occurring abroad, shew that the synchronism of shocks at distant localities, when carefully examined, wants the accuracy and consistency necessary to prove a common origin. Two well-known *foci* of disturbance are St Jean de Maurienne in Savoy, and Comrie in Scotland, separated by an interval of 1100 miles. It happens sometimes that an earthquake occurs at both on the same day, and the coincidence is thought remarkable. But on comparing a register of the shocks at each for the five months from October 1839 to March 1840, Mr Milne found that while no less than 150 were observed at Comrie, and 58 at St Jean de Maurienne, there was a complete want of that general agreement which a common origin would have produced.* Indeed, when we are apprised of the frequency of the shocks at each locality, we naturally infer that an *accidental* coincidence within the limits of a day must occur at times. Mr Milne, therefore, considers the evidence he has collected as irreconcileable with the idea that any connection exists betwixt the sources of subterranean movement at these localities. Other observations have led him to conclude, that even in cases where the seats of disturbance are comparatively near (as Comrie and Oban), their action may be independent of each other. The agitation felt over so wide a region at the great earthquake which destroyed Lisbon, and on other occasions, may be considered as a mere vibration propagated through the outer part of the crust, from the focus of disturbance, like the tremor which accompanies the blasting of rocks. From the independence of action, he reasonably infers that the seat of disturbance cannot be at any great depth. These views harmonise with the conclusions of Mr Hopkins, deduced from very different data. While, therefore, some of the basins of fused matter may be of great extent, like that under the Andes (supposing the synchronism of volcanic action over the long line to be established), others may be very small; and the simultaneousness of disturbance at distant spots may be merely the effect of vibrations propagated from the centre.

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* Edinburgh New Philosophical Journal. No. 72, p 363.

I shall advert in the briefest terms to some hypothetical views thrown out by Mr Hopkins, as to the changes of form the globe has undergone. If the earth was originally fluid, it might pass to the solid state in two modes. The heat would be continually dissipated from the surface, and would therefore be greatest at the centre; and so long as the mass was fluid, the inequality of the heat would cause a constant circulation betwixt the surface and the centre. Now, if the effect of heat in preventing solidification was greater than the effect of pressure in promoting it, solidification would begin at the surface, where a crust would be formed, and would constantly increase in thickness, by layer after layer added to its under side. But if the effect of pressure in promoting solidification was greater than the effect of heat in preventing it, solidification would begin at the centre and extend outwardly. While the process was going on, circulation would continue in the fluid part exterior to the solid nucleus. But before the last portions become solid, a state of imperfect fluidity would arise, just sufficient to prevent circulation. The cooled particles at the surface being then no longer able to descend, a crust would be formed, from which the process of solidification would proceed far more rapidly downwards than upwards from the solid nucleus. Our globe would thus arrive at a state in which it would be composed of a solid exterior shell, and a solid central nucleus, with matter in a state of fusion betwixt them—a state, in short, similar to that indicated in figure 2, supposing the central space, N, to be filled up with solid matter.

On the Terrestrial Arrangements connected with the Appearance of Man on the Earth: being the substance of a Lecture delivered by Professor GUSTAV BISCHOF of Bonn, at Bonn.

I. *Coal and Soil.*

Coal. Evaporation goes on the more rapidly the higher the temperature of the sea and of the surrounding atmosphere. The southern seas are, therefore, much more productive of vapours than those situated farther to the north. Moreover, during the earliest geological periods, when the amount of heat diffused over the earth was comparatively greater than at present, the quantity of atmospheric moisture must have been much more considerable.

We have, in one of our former lectures, pointed out the surprising grandeur and luxuriance which characterised the vegetation that was destined to furnish the materials for the formation of our immense beds of coal. Whence comes this luxuriant growth of plants? Because the two main conditions of vegetable life, heat and moisture, were then much more copiously diffused than they are at present.

It has also been observed in one of our former lectures, that the ocean covered a much larger space ages ago; and that extensive countries now raised above the sea, were then but inconsiderable islands. The ocean yielded a greater amount of vapours, not only because it was warmer, but because it presented a larger surface. These, then, were the principal causes active in the production of a very great early vegetation. It has been incontestibly proved, that at one time the whole earth, with the exception of a few islands, must have been covered by the waters of the ocean. We shall here make a few remarks on this subject. If, for instance, our Rhine province had formerly possessed the same extent of surface as at present, we would feel completely at a loss to account for the fact, that the coal strata are so irregularly distributed over the country.

The same causes which, in the vicinity of Saarbrücken, of Eshweiler and of Aachen, gave rise to a luxuriant vegetation, and which influenced the formation of such extensive beds of coal, must have been in operation all over the other districts of the Rhine province. Instead of this, the coal has been deposited in isolated basins, analogous to the manner in which islands are grouped together. Some of these coal-beds are, however, of considerable dimensions; witness, for instance, the enormous coal-beds in England and Scotland, which prove such a blessing to these two countries, and which lead us to infer, that, during the period of luxuriant vegetation, Great Britain had nearly attained to its present size.

Another component element of our atmosphere, viz., carbonic acid gas, formerly in large quantities, and which constitutes a chief portion of the nourishment of plants, in conjunction with heat and moisture, acted a prominent part in the production of a vegetation remarkable for its luxuriance.

On examining more closely, we cannot but perceive the admirable order displayed, in all the arrangements of nature. This very element of our atmosphere, the carbonic acid, so indispensable to the growth of plants, is prejudicial to animal life; for an atmosphere containing more than 8 per cent. of this gas proves fatal to every animal, our own species not excepted. It kills, because it arrests the process of breathing. Nevertheless, the elements of which it is composed are necessary to the sustenance of human life. Not a single animal has been gifted by nature with the faculty of digesting these elements when presented under this particular form. Vegetation was destined to inter-mediate between unorganized nature and the animal world. An unorganized world issued from the hand of the Creator; immense quantities of carbonic acid gas were disengaged from its bowels. This gas was decomposed by plants, the second wonder of the creation; and food was thus provided for animals, the third wonder of the creation.

For what reason was it that warm-blooded animals did not make their appearance when such enormous quantities of food lay ready for consumption? Because the colossal vegetables were destined to purify the atmosphere, and to reduce the carbonic acid gas to a certain minimum. (The average proportion in which it occurs in our present atmosphere is nearly one in 2000 parts.) But they were also destined to furnish the materials for fuel and commerce. The next period brought to light the various species of monstrous reptiles; the gigantic lizards, and others. All the conditions necessary to the growth and propagation of these monsters were then in existence: abundance of food, and an excess of heat and moisture. The atmosphere at that time —in a state of much greater impurity than it is at present— could have no effect on these reptiles, accustomed as they were to breathe the foul air of swamps and marshes.

It was for the exclusive benefit of mankind that these early vegetables were converted into dead matter, so as to furnish the materials for coal. We are always in the habit of considering the material world created for no other object than that of ministering to our own immediate wants and pleasures. We fancy ourselves the lords of the whole creation; and it is, therefore, natural to ask for what purpose was it that such a vast number of animals were first created, and these again destroyed—what part in the great drama of life was to be performed by those large reptiles? We might answer with a verse from Ecclesiastes (i. 4), "One generation passeth away, and another generation cometh; but the earth abideth for ever." Let us, however, be more explicit on this subject.

In the first place, let us put the question in general terms. Why have millions and millions of animals been doomed to live and to die before man could make his appearance on earth? The answer is very simple. The Brahmins live exclusively on vegetable diet. This proves that we can exist without the flesh of animals. There are many amongst us who, in imitation of monastic discipline, abstain altogether from animal food. We might thus be induced to believe that human existence is independent of animals. It is easy to expose the fallacy of this reasoning. We assert that those who feed upon vegetables only, belong, nevertheless, to the class of carnivorous animals.

This looks very paradoxical, but still it is true, We may, indeed, live on mere vegetable diet, provided that the plants have grown on a soil manured with the dung of animals; but the dung of animals implies the existence of these latter. Animals were, therefore, of necessity the precursors of the human race. Animals are said to be either carnivorous or herbivorous. We may, with equal justice, express ourselves in this manner: every animal is both herbivorous and carnivorous. Our horses and our cattle are classed among the herbivorous animals, but their food is produced on a soil fertilized by the dung of animals. Although we are not in the habit of manuring our pasture lands, it is easy to prove that the growth of the grass depends entirely on the presence of animal manure, which, if not actually mixed with the soil, is carried to the plants by the atmosphere.

As it is very evident that the world of animals has emanated from that of vegetables, it follows that the first race of animals inhabiting our earth were purely herbivorous. It would, however, be difficult to point out the exact species. I merely wish to draw your attention to the fact, that when plants, of whatever description, are made to pass into a state of putrefaction by keeping them immersed in water, a crowd of animalculæ—the so-called Infusoria—is then brought to view by the microscope. The same mysterious laws, which cause animals to spring up under our own eyes, were likewise in operation at the period when the earliest race of animals was called into being. With the infusoria the first link of the great chain is given, connecting one generation with another, until it closes with our own species, the last and most perfect of created animals.

All that is required are infusoria—dating their birth from the putrefaction of vegetable matter—in order to obtain a series of carnivorous animals.

The moment that plants began to decay, and to give rise to infusoria, which, in their turn, fell a prey to other small animals—for instance, to the mollusca, which again became the food of a larger species, &c.—they became part of the food of the monstrous reptiles, the most voracious of the then existing animals; that moment organization had taken a new direction.

Ages ago, when by far the greater portion of vegetables was converted into dead matter for the formation of coal, there was but little left for the food of molluscous and other small animals. The converse appears to have taken place in a later period. The red and variegated sandstone formations, and the groups of oolites, where those monstrous reptiles are still found in a fossil state, is indeed productive of coal, but the beds are very thin and few in number. On the other hand, the remains of animals are copiously disseminated throughout the whole mass of the rocks just mentioned. These remains, the result of decayed animal and vegetable substances, and of very common occurrence in the various kinds of sandstone, are all comprised under the term Bitumen. Accordingly, we read in geological works of bituminous slate, of bituminous limestone, &c. In the copper slate, which is a formation very widely distributed, and where the working of mines proves to be a lucrative business—as, for example, at Stadzbergen, in the province of Westphalia—the bitumen amounts to the tenth part of the weight. This slate abounds with the impressions of fish, from the substance of which the bitumen has for the most part been derived. The contorted position frequently indicated by these impressions intimates a violent and sudden death of the animal; and their complete preservation proves, that, soon after death, the fish were imbedded in a mass of finely divided mud.

In a similar manner, the colouring principle of the most esteemed species of marble, embracing the spotted and striped varieties, as also those of a yellow, red, brown, or blackish colour, consist exclusively of bitumen. Hence it happens that all these species burn completely white—the bitumen is destroyed, and the white limestone remains. The drawing slate (black chalk) used by artists is likewise indebted to bitumen for the blackness of its colour.

The manner in which animal substances are transformed into bitumen is very plainly illustrated by the ammonites—a genus of shell abounding in the lias formation. Among the vast number of ammonites found in the lias, we have had occasion to examine several where the large external chamber forming the abode of the animal is found half empty. The creature in its death-struggle seems to have, as far as possible, retreated into this part of the shell, so as to prevent the mud from entering. The matter which occupies the other divisions of this latter chamber is, owing to the decay of the animal, highly bituminous.

Soil. If we now consider, that all the mountain strata, formed at a time when billions of animals might easily be buried in their substance, are filled with their remains, we may justly regard these strata as the large cemeteries or burying-grounds of antiquity, if we be allowed to use such an expression. The greater portion of the crust of our globe is formed by these strata. Let us, for instance, examine the mountains of Switzerland and of our own country. In the Jura mountains of Switzerland the strata rise to a height of from 4000 to 5000 feet above the sea; they continue their course through Swabia and a part of Bavaria, as far as Saxe-Coburg, reappearing in the north of Germany between the Weser and the Hartz mountains. Similar strata are found in Swabia and the northern parts of Germany, and amongst them the red sandstone formations occur in considerable masses.

Suppose the surface of all these strata to be decomposed by the action of the atmosphere, and to be converted into earth, what will be the result? We obtain a mould or soil impregnated with primordial manure. All those animal and vegetable substances, which have been imbedded in these mountain strata during the period of their formation, are there still, existing, in a mineralised condition, under the form of bitumen; for not a particle of matter can be lost. Since the Creation, there has not been lost one single grain of sand, nor one single drop of water. There is only motion in a circle,—one metamorphosis succeeding another. It follows, therefore, that all those mountain strata, which abound with the remains of animals and vegetables, furnish a species of rich soil. Plants and fruit-trees thrive and give nourishment to man and beasts, at the expense of these remains and of this primordial manure. We return to the fields, through the medium of manure, what we gather from them at the various seasons. Again a motion in a circle.

Nature, in order to distribute the fertile mould over the country, and to carry it even to the lower plains and sandy deserts, has raised into a vertical position the strata so often alluded to, and which were originally deposited at the bottom of the sea.

They have been raised to heights exceeding 10,000 and 12,000 feet. I shall here advert to one particular mountain. Six years ago, as I was ascending the Faulhorn, which is situated in the highlands of the Canton Bern, and rises about 8,200 feet above the level of the sea, I inquired of my guide concerning the origin of the name given to that mountain. His answer was, because the rocks of which it is composed, are more apt to rot than any others in Switzerland. This was not correctly expressed; because stones cannot undergo the process of rotting. I understood, however, what he meant to say; and I became soon enlightened on the subject by ocular inspection. The mountain is formed of a species of slate of a blackish colour, which is easily decomposed by the action of the atmosphere. The water insinuates itself between the laminæ, and expanding, when in the act of freezing, tears the rocks asunder; so that, on the commencement of thaw, large masses of stone are seen to roll down into the valleys below, where they break to pieces, are decomposed, and finally dissolved into a mould of a deep black colour. On lifting up stones of the size of my fist, or larger, they appeared so soft to the touch, that I could easily reduce them to powder between my fingers. Towards the end of August, I spent a few days on the top of this mountain (the highest in Switzerland, where the traveller can be accommodated with lodgings), with the view of making experiments. It was one of my objects to observe the temperature of the ground in such an altitude. I hardly expected to accomplish this, as it was necessary to fix the thermometer in the ground to the depth of nearly one foot, a thing quite impracticable on heights consisting of solid rock. To my astonishment, on removing the snow, I could, with the greatest ease, penetrate the ground to the depth of several feet, where I discovered a mould so rich, and of a colour so intensely black, that I would think myself fortunate to have some of it in my garden. There, on the borders of eternal snow, we might rear the most delicate garden-plants, were it but possible to provide them with the necessary quantity of heat. This valuable humus was evidently derived from the decomposition of the strata of the black limestone rock, of which the Faulhorn chiefly consists.

The mountain-torrents, when swelled by a continuation of rain, or by the melting of snow, carry this fertile mould from the mountain to the lowlands. The Bergelbach, one of the largest, is charged to such a degree with this finely divided earth, that the water has assumed a deep black colour, and that it communicates this tint to another glacier stream, somewhat the size of our Sieg, which, on that account, has received the name of the Black Lütchine.

The productive powers of this mould are displayed to advantage on taking the rather dangerous route from the Faulhorn to the Giessbach, a celebrated waterfall in the vicinity of the Lake of Brience. As soon as we pass into the region of forests, we encounter the most luxuriant vegetation, not surpassed by that of tropical countries. The tallest fir-trees are there crowded together, improving the soil by their decay, and thereby clearing a space for the growth of others. The traveller forces his way with great difficulty through the shrubs, and across an ocean of the most delicious strawberries, raspberries, and bilberries, &c.

What enormous quantities of the most fruitful soil have been transferred from this mountain alone to the lowlands, through the agency of the mountain-torrents! And this has been going on for centuries, and will continue for thousands of years, until, in the course of time, the whole Colossus, now at an elevation of 8200 feet above the level of the sea, shall have entirely disappeared.

After such reflections, we need no longer be astonished at the fertility of the valley of the Rhine, for which it is indebted to Switzerland. What wonder, if, by the accumulation of a fertile mould, which, for thousands of years, has been floating down the Rhine, entire countries, such as Holland, have, out of large plains covered by the sand of the sea, been converted into the most fruitful corn-fields and pasture-lands? Indeed the Dutch ought to pronounce the name of Switzerland with the greatest respect, for Holland owes its existence altogether to Switzerland.

My friend, Von Dechen, has informed us, that the waters of the Rhine rose formerly to a much higher level than at present. At that period, the river deposited a species of earth of a yellowish-white colour, which bears the name of marl (in German, *Löss*). This earth may be seen to the right of the public road between Remagen and Sinzig, extending in compact masses high on the neighbouring hills. It occurs, moreover, on the road from Poppelsdorf to Typendorf. It is likewise a gift of Switzerland; though many tributaries of the Rhine, rising in the Schwarzwald, Overwald, &c., come in for a certain share. The vegetation between Remagen and Sinzig proves it to be a mould possessing strong productive powers, though inferior to that of the Faulhorn and other mountains of Switzerland.

II. *Saltpetre.*

Chemical experiments have demonstrated, beyond all doubt, that saltpetre, a well-known salt, requires for its production the presence of animal remains. From time immemorial this salt has been procured from Egypt, the East Indies, &c.; formerly in smaller quantities, but since the invention of gunpowder, of which it is the chief element, its importation has become very considerable. In those hot countries, the salt effloresces on the surface of the ground. The species of rock from which it is secreted, has recently been examined in the island of Ceylon, where saltpetre is of frequent occurrence, and the result has shewn it to be a limestone containing animal matter. Mariano de Rivero discovered, not many years ago, immense quantities of a similar salt, the so-called *cubic saltpetre*, in the wilds of Atacama, a province belonging to Peru. The bed which it forms is overtopped by a thin coating of earth, and extends in one direction for upwards of an hundred miles, in beds of variable thickness. There is not the least doubt that a multitude of animals found their grave in this quarter.

How singular, that one race of animals was doomed to perish, in order to furnish, in such vast quantities, the materials for the destruction of other races of animals; and we grieve to think even for the slaughter of human beings when engaged in deadly warfare. But whatever may be the pernicious effects of saltpetre when employed under the form of gunpowder, it is impossible to do without it in the present advanced state of society. The want of gunpowder would prevent us from conducting roads through rocks and over large mountains, and from building tunnels for the use of railroads. Without saltpetre, chemistry, which so powerfully influences our trades and commerce, would scarcely have existence.

After all these reflections, is there still need of asking, why were such multitudes of animals destined to live and to die before man could make his appearance on earth?

What a miserable doom, one might exclaim, was imposed on the extinct races of animals, to live merely in order to perish! But what other fate awaits the present race of animals? What difference is there between the slaughtered ox and those monstrous reptiles which, millions of years ago, were suffocated in mud? Merely this, that the flesh of the former is directly used for food, whilst that of the latter was fitted for our nourishment only after a series of metamorphoses. I have remarked, in my last lecture, that nothing on earth exists for its own sake, but that every thing is created for the attainment of higher objects. Even man himself is but a link in the great chain of events. The moment he begins to care for nothing beyond his own self, he ceases to be a useful member of society. It is our duty to employ our talents and our skill for the good of our fellow-creatures; and, as regards the lower animals, we consider them bound to serve us with their physical strength, and with their body.

III. *Water—its Effects.*

On casting another glance upon those long periods which my colleague, Mr Goldfuss, has so well described, we cannot but perceive, that when Divine Providence caused a vegetation to spring up for the subsequent deposition of coal-beds, it was with the view of supplying us with fuel and the means of preparing our food. Nature made use of the then superfluous heat by expending it on the growth of a luxuriant vegetation, and afterwards of a vigorous animalization. This was a very wise arrangement in the economy of nature. She, in order to store up a portion of the original heat for the benefit of the future race of man, buried, in the bowels of the earth, the whole mass of vegetables which had been reared by the aid of a high temperature. A pious mind cannot but feel deeply moved, on contemplating the infinite wisdom and goodness of the Creator, which is manifested in the works of nature. "O Lord," so we may exclaim with the Psalmist (Ps. civ. 24), "how manifold are thy works! in wisdom hast thou made them all: the earth is full of thy riches." I have already observed, that the temperature and the waters of the sea have always been on the decrease since the period characterised by the growth of a monstrous vegetation. What was the consequence? This decrease of the temperature and of the waters of the sea involved a decrease in the amount of vapours, which arise from the latter, and descend again, under the form of rain. In order to remedy this, and to restore the balance, it became necessary to raise up chains of lofty mountains.

It is a well-known fact, proved even by our hills, the Siebengebirge, and the Slate Mountains of the Rhine, that a greater quantity of moisture is condensed from the atmosphere by the action of mountains, than by that of plains.

We observe, that the clouds are attracted by the mountains, that they discharge upon them their contents, and give origin to springs, brooks, and rivers. It is said in the same Psalm, that the Lord sendeth the springs into the valleys, which run among the hills.

It was sufficient to raise the chain of the Alps in order to supply with water, through the medium of the largest streams, a considerable portion of Europe,—the south and west of Germany, the Netherlands, the south of France, the north of Italy, Hungary, and European Turkey.

Those parts of the Alps which have been lifted above the snow-line, became, of necessity, covered with eternal snow. There was nothing lost by this arrangement, though large tracts of country were thus rendered inaccessible to the growth of plants and animals; for beyond a certain altitude there is an end to every species of organized products. Add to this, that on the other side of the Alps a large extent of country, traversed by low ranges of hills, is well adapted for the growth of organized products. A fertile mould, covering the ground for miles, and extensive tracts of Alpine country, where numerous flocks of cattle and goats are seen to pasture, became the result of those subterraneous actions by which nature has uplifted mountains. Fertility was gradually spread from the Alps to the most distant countries of Europe. The heat engendered in the narrow and deeply indented valleys of the Alps would become intolerable, and forbid the growth of plants, unless the atmosphere were constantly cooled down by the neighbouring snow and ice-mountains as also by the ice-cold waters of the glaciers.

In the same way as the superfluous heat of former ages has been, as it were, preserved by the coal-beds, the water which, during winter, falls down in the form of snow, is stored up in the Alps for the summer season. Glaciers descend from the highest parts of the Alps, which lie buried in everlasting snow, into those regions where the snow begins to melt in summer. At the same time that those rivers, which do not rise from the Alps or glaciers, as, for instance, our Elbe, Oder, &c., are nearly dried up during the summer months, the streams issuing from the Alps, as, for instance, the Rhine, the Danube, the Rhone, the Etch, &c., continue to swell in proportion as the heat increases; for the greater the heat the larger will be the supply of water, formed by the melting of the snow and of the ice of the glaciers. Nature has covered the Alps with eternal snow and ice; but she avoided to do so with regard to the inferior regions of lakes and of the sea, because she intended them for the abode of organized beings. To what expedient did Nature resort, in order to effect her object? She fell upon a very simple plan, but which appears, on that account, so much the more wonderful.

All substances, both in the liquid and in the solid state, contract during the process of cooling; and the more so the longer that process is carried on. We may observe this every day on the liquid mercury contained in the glass tube of our thermometers. We perceive that the column contracts whenever the cold increases. The thermometer is then said to fall. The contraction of water is, however, regulated by a law very different, and very peculiar. It is certainly true that water contracts in proportion as the cold increases; but the instant that it has cooled down as far as 39° Fah., it ceases to contract,—nay, at a still lower temperature, it begins again to expand, and continues to do so down to the freezing point. The power of expansion is so considerable, that the strongest metallic vessels, if completely filled with water, and closely shut, are seen to burst during the process of freezing. This power is indeed irresistible. I request you to keep this in remembrance, since I shall afterwards have occasion to revert to this subject.

Water diminishes in volume, and gains in specific gravity, in proportion as it continues to contract. Again, a heavier fluid sinks below that which is lighter, as may be witnessed on pouring water on oil. In the same way the heavier particles of water descend through those which are lighter, and the lighter ones rise through those which are heavier. What takes place in a lake, for instance, in our Laacher Lake, on the commencement of the winter's cold? The sheet of water on the surface being in immediate contact with the cold atmosphere, begins to assume a lower temperature. It contracts, becomes heavier, and sinks down through the water below, which, being warmer and lighter, rises in its turn to the surface. This movement continues, until the water which is uppermost has acquired the temperature of about 39° Fah.; its specific gravity is then at its maximum. Water of this temperature has, therefore, still a tendency to sink; but it loses that tendency the moment that it cools down below 39° Fah.; for now it begins again to expand, becomes lighter, and swims on the warmer water below, as oil swims on water. It follows from this, that water of a temperature lower than 39° Fah., can never reach the bottom of the lake. We have thus explained the mystery, why deep lakes can never be frozen to the bottom. The temperature of water, which occupies the lower regions of lakes, can never sink below 39° Fah.; whence we infer that, at a certain depth, there exists a temperature of about 39° F., and this not only in winter, but likewise in summer. I have said likewise in summer, because it is obvious that water of the above temperature can never be replaced by water of a higher temperature, on account of the inferior weight of the latter. Deep lakes exhibit, therefore, this peculiarity, that heat cannot descend downwards, whereas cold may. But as it is impossible for water of an icy temperature to arrive at the bottom of the lake, it follows that the lake cannot be frozen to the bottom.

Many experiments, made, for instance, in the lakes of Switzerland, prove the truth of our theory. On examination, the temperature of their lower regions amounts at all seasons to from 41° Fah. to 43°.2 Fah. The cause why it was never exactly 39° Fah., is attributable partly to the internal heat of the earth, partly to the circumstance that water of the temperature of 39° Fah. never reaches the bottom without being mixed with some of the warmer particles through which it passes. This temperature of 41° Fah. or 43° Fah. is observed in all the lakes where that of the surrounding atmosphere sinks in winter at least as low as 39° Fah. It is common to all the lakes of the northern and southern countries of Europe; as, for instance, to the lakes of Sweden, Norway, and Lower Germany, as well as to those of the Alps and of Italy. Hence it is intelligible why the same species of fish are found in lakes belonging to very different climes. The unequal temperature of the atmosphere does not in the least affect them. The fish inhabiting the lakes in the north of Sweden swim about in their native element at a depth where the water has constantly the same temperature; as is, for instance, observed in the Lago di Como, although in winter the atmosphere frequently shows 20° or 30° below 32° Fah., whilst in summer it rises here as many degrees above 32° Fah. It is only during the hot season that the fish betake themselves to the upper regions, in order to deposit their spawn.

The same providential care which Nature has bestowed on the accommodation of the finny tribe is also discernible in the manner in which she has attended to the comforts of quadrupeds. The organization of each particular class is strictly adapted to the climate and condition of the country assigned to it. The ice bear and the reindeer are confined to the polar regions; the lion and the leopard to the torrid zone. Misery and death await them should they venture beyond the bounds of their native clime. Birds, the most nimble of all animals, are by nature allowed the most extensive range. The birds of passage—as swallows—too delicate for the severity of our winters, leave us in autumn, in search of warmer countries. Reptiles—as toads, lizards, serpents, &c.—not provided with the means of escape, hide themselves in the bosom of the earth, to protect themselves from the winter's cold. The insects, which in summer swarm about in such abundance, perish at the commencement of winter, but their eggs and larvæ are preserved for the propagation of their species. How very different from this is the life of fish, allowed to traverse their native element at a depth where they may always enjoy the same uniform temperature.

Suppose, now, that the creation of water had been left to ourselves—short-sighted beings as we are—with what properties would we have endowed it? It would never have occurred to us, in the case of the contraction of water, to deviate from the general law with regard to the contraction of bodies. Like other fluids, we would have made it to contract as far as the freezing point. What would have been the consequence? In one severe winter the beautiful lakes of the Alps, and of other countries visited by frost, would have been frozen to the bottom. The fish, and every other creature in them, would have died—a whole creation would have perished. Nothing is plainer than this.

It is evident that lakes, no deeper than the Rhine, will require the same time for cooling down to 32° Fah. The temperature of that river sinks to 32° Fah. a few days after the commencement of frost, when shoals of ice are seen to float about. For a series of winters I have been in the habit of examining the temperature of the Rhine at the time of incipient frost, when I have invariably found that the thermometer, although it stood several degrees above 32° Fah., fell to the freezing point upon the weather continuing severe for three days. You may make the same observation, with less inconvenience to yourselves, if you watch the Rhine from your windows. Mark the day when the first ice is seen on the streets. On that day you will never perceive any ice floating on the Rhine. This will, however, be the case after a few days of sudden and intense frost.

But it is not only on the surface that the Rhine assumes the temperature of 32° Fah.; it may be traced at whatever depth we examine it. Several years ago this matter was very carefully investigated at Strasburg. Water, drawn from different depths, shewed the same temperature of 32° with that on the surface.

In some places the Rhine is more than fifty feet deep. This river being frozen three days after the commencement of severe frost, it follows that a lake 1500 feet deep, for instance the Lake of Geneva, will cool down to 32° in the course of three months; so that the next moment it may be converted into one solid mass of ice. Considering that in the Alps, where the lakes occupy a much more elevated situation, the winter makes its appearance in November, and frequently lasts till April or May, it is evident that such lakes will already be frozen to the bottom before the end of February. It is true, that, in the succeeding summer, the ice would begin to melt on the surface, but that would scarcely produce a sheet of water a few feet deep; for, in order to melt a mass of ice 1500 feet thick, it would require our summer heat to continue without intermission for many years. Such lakes would cease to deserve the appellation of lakes; they would for ever present one solid mass of ice.

Such would have been the fate of the magnificent lakes in Switzerland, in Upper Bavaria, and in Upper Italy; of the charming Lago Maggiore, of the Lago di Como, and others. Their fish would have been frozen to death, and their shores stripped of that matchless luxuriance of vegetation for which they are so remarkable. Steam-boats would have been out of the question, for the thin sheet of water obtained by the melting of the upper crust of ice would scarcely admit of the use of flat canoes.

Our beautiful lakes in Northern Germany, for instance those of Brandenburg and Mecklenburg, and which are almost the only ornaments of those countries, would be visited by a similar misfortune. We might certainly, up to the middle of summer, amuse ourselves with skating, and with excursions on sledges; but then the ice would melt so slowly as to leave the lakes scarcely accessible to the smallest boats.

How different would be the aspect of countries if water had not been endowed with the peculiar property of attaining its maximum density at about 39° Fah. In the contrary case, nothing would have been better, but everything so much the worse; and we ought, therefore, to give praise to our Creator, who, by such simple means, has conferred on mankind such great and everlasting benefits. Job, the hero of that well-known ancient poem, seems to allude to this when he says, chap. xxxviii. 29, 30, "Out of whose womb came the ice? The waters are hid as with a stone, and the face of the deep is frozen."

The atheist may object to this, and protest that the water received this property by a mere caprice of nature. But what right have we so to call that beautiful arrangement, whereby such important ends are accomplished? He who does not recognise therein the power and exceeding mercy of God, will never find it elsewhere.

Let us now turn away from that picture of desolation, and once more direct our attention to contemplate that wise arrangement by which such great things have been effected. A continued frost is requisite, in order to reduce the temperature of deep lakes to 39° Fah. If the frost continues still longer, a thin layer of water at the surface begins to undergo the process of freezing. The crust of ice that is forming slowly increases downwards, but, on the appearance of thaw, its growth is immediately arrested. Under this cover the fish continue in a lively and active condition, because the region in which they move about preserves, winter and summer, the same temperature. A few warm days of spring are sufficient to melt the ice, and to destroy every trace of the winter.

The time which is required in order to cool the lakes down to 39° Fah., and to continue that process on their surface, is proportional to their depth. The freezing of deep lakes is, therefore, a very rare occurrence. It has happened but once within these fifty-four years, namely, in the year 1830, that the Lake of Constance was frozen over during the severe frost in January and February. It was almost completely covered with ice, with the exception, however, of a small circle opposite to Frederichshafen, which, being exactly over the spot where it is deepest, presented an open space, scattered over with shoals of floating ice. The greatest thickness of the ice was found to be half a foot. Of course, a few warm days of spring were sufficient to remove all traces of the ice.

The sea presents relations very analogous to those of lakes; But there is this difference, that the water of the former, owing to its salt condition, takes much longer time to freeze than that of the latter.

I have stated before, that water expands in the act of freezing, and that this power is irresistible. It follows from this, that ice must be lighter than water; for it is seen to swim on the latter.

Here we have again a proof of Divine wisdom. What would be the consequence if the ice had been heavier than water? That substance would sink to the bottom as soon as it is formed, a second layer would thus be deposited, and so on, until the whole bed of the Rhine was filled with ice. An impenetrable and immoveable ice-wall would thus begin to overtop the water, cause the river to overflow, and to deposit fresh masses of ice. The ice-wall would then rise above the banks, occasioning the most fearful inundations, by which the whole country would be converted into one scene of desolation and misery.

If it had pleased the Almighty to ordain that ice should be heavier than water, one single severe winter would be sufficient to destroy all our cities, and to lay waste all the adjacent districts of the Rhine. The beautiful valley, which extends along the whole course of that river, from Switzerland down to Holland, would present one entire wilderness.

But do not imagine that ice has been made lighter than water, merely in order to prevent ruin and desolation! Nature intended thereby to confer on us the most signal and everlasting benefits. The whole coast extending from Holland to Russia is deficient in rocks. In that whole direction, not one single rock is to be seen. In order to remedy this evil, Nature fell upon the following plan: At an early period of our earth, she loaded large ice islands with the rocks of Sweden. This was probably the same period when the mammoths and elephants were buried in the ice at the mouths of the river Lena. The ice islands landed on the coast of the Baltic, then still under water; they divested themselves of their burden, depositing the stones in localities where they are now found under the name of *erratic blocks*, consisting chiefly of large pieces of granite, porphyry, &c., and employed for decorating the bridges and museums of Berlin, as also for paving the highways and public roads of Brandenburg. Our countrymen on the Baltic enjoy the possession of these stones merely because ice is lighter than water.

You will remember that I stated before, that the expansive power of water, when in the act of freezing, is irresistible. Nature possesses no gunpowder, for this is altogether an artificial product; but she accomplishes by water what we obtain by fire. I have already brought to your notice, how the rocks of the Faulhorn are broken to pieces by the action of freezing water. Nature purposed to convert steril rocks and stones into a fruitful mould. She employed the simple means of admitting water into the crevices, and of causing it to freeze. The ice, in severing the rocks, acts on the principle of a wedge. When it begins to melt, it assists in loosening the rocks, and in accelerating their dilapidation. This process is repeated until the stone is completely reduced to clay.

I have now endeavoured to shew, that Nature is able to accomplish great things by small means; and I trust you will not depreciate the small means I have employed in order to amuse you, and to direct your attention to a class of phenomena on which you have had perhaps little occasion to reflect. It has fallen to the share of very few individuals to perform great things by small means; an ordinary mortal frequently accomplishing but little by great means. But he is contented with the testimony, that his labours have not been thrown away.

Contributions towards Establishing the General Character of the Fossil Plants of the genus Sigillaria. By William King, Esq., Curator of the Museum of the Natural History Society of Newcastle-on-Tyne, &c. (Communicated by the Author.)

(Continued from page 390 of vol. xxxvi.

Hitherto no specimens of the genus *Sigillaria* have been made known, possessing clear evidence as to the nature of its root. In a few instances, this organ is said to have been found attached to the stem of this plant; but, with the exception of a single case, to be alluded to presently, the *external characters* were so imperfectly displayed as to render their identity with those of any known fossil a matter of complete uncertainty. The Sigillarias found at Dixonfold, in the excavation of the Manchester and Bolton Railway, though clearly shewing the junction of the stem with the root, yet, from the absence of the requisite characters, the latter part cannot be identified with any of the coal-measure fossils; even the stem is in such a state as to shew, in no very satisfactory manner, its identity with *Sigillaria.* The same may be said of the fossil which is described by the Rev. Patrick Brewster, in a paper read at a meeting of the Royal Society of Edinburgh, in 1818, and published in the sixth volume of their Transactions. The Killingworth specimen described by Mr Nicholas Wood, in the first volume of the "Transactions of the Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne," and by Lindley and Hutton, in the first volume of the "Fossil Flora," appears to have afforded clearer evidence on this point; but, unfortunately, some cause or other, prevented it at the time, being followed up so completely as could be wished. The fragment figured by these gentlemen, as a portion of the root of this specimen, is now in the Newcastle Museum; and it is a fact not generally known, that it is no other than a true *Stigmaria.*

Our attention having been drawn to the last genus, we will, in the next place, confine ourselves to the same, with the view of ascertaining whether or not its connexion with *Sigillaria,* as favoured by Wood, Lindley, and Hutton's account of the Killingworth fossil, can be supported by any other evidence than such as they have published.

With reference to the situation which *Stigmaria* occupied in the vegetable kingdom, various conjectures have been formed. Naw considered it a palm: Schrank allied it to the Stapelias: Von Martius and Sternberg thought that it approached to the Euphorbias and Cactuses,—an opinion which Lindley and Hutton seem inclined to adopt: Brongniart at first placed it in "*Aroideæ,*" afterwards he referred it to "*Lycopodiaceæ;*" but, of late, he considers that it ought to be included in "a peculiar and extinct family, belonging probably to the Gymnospermous division of the Dicotyledons:" Buckland, in his "Bridgewater Treatise," seems in favour of its euphorbiaceous affinities; but he advances the opinion that it was probably an aquatic plant, "trailing in swamps, or floating in still and shallow lakes, like the modern Stratiotes and Isoetes:" Corda considers it to be more or less connected with "*Crassulaceæ*" and "*Cycadaceæ;*" Gœppart elevates it to the rank of a family under the name *Stigmariadæ,* and looks upon it as connecting the Lycopods with the Cycases: and Endlicher, in his "Enchiridion," places it in the order "*Isoeteæ,*" class "*Selagines,*" which, in addition to the latter, is made to include the Lycopods and Lepidodendrons.

The earliest detailed account of *Stigmaria* was given by the Rev.

Henry Steinhauer,* who described it under Martin's name "*Phytolithus verrucosus.*" He supposed it to have been a "cylindrical trunk or *root growing in a direction nearly horizontal, in the soft mud, at the bottom of fresh-water lakes* or seas, without branches, but sending out fibres on all sides."

The next account of this fossil appeared in the "Fossil Flora," by Lindley and Hutton, who have expressed themselves much in the same terms as Steinhauer, respecting its habitat and mode of vegetating; but instead of a branchless cylindrical trunk, as Steinhauer supposed, their view is, that it had a centre, in the form of a "continuous homogeneous cup or dome," from which "numerous arms proceeded on all sides."

In connection with the specimen which has led to the foregoing opinion, and which Messrs Lindley and Hutton have figured in their "Fossil Flora," † a point must, in the next place, be considered, which, if not cleared up, will leave the question now entered upon completely inconclusive, however cogent the arguments may be that are to be adduced.

As the specimen itself has not been fully described,—and as it is fortunately preserved in the Newcastle Museum, though not exactly in the same condition as when first obtained,—the present opportunity may be embraced to state, that it is a convex mass of shale about four feet and a-half in diameter, and fifteen inches high in the centre: the crown or central part, which may be reckoned two feet across, is evenly rounded, and the sides are channelled: the whole of the crown is crowded with strongly marked wrinkles, which pass off into the channels on the sides: in a few instances, a channel is occupied with a compressed branch, also composed of shale, and encircled with a thin layer of coal—in this case, the remains of a cuticle; with this exception, the specimen is completely decorticated. It is necessary to observe, that the strong wrinkles of the crown become much finer as they pass off into the channels; and that there is superadded to the latter a number of scars, which, as well as their accompanying wrinkles, are in every respect similar to those of *Stigmaria:* both characters were doubtlessly impressed by the outer surface of the cuticle of the branches. After alluding to some other specimens which had been discovered in the roof of the Bensham seam in Jarrow colliery, one of which is described as shewing a central concavity and fifteen arms proceeding from it, and consequently resembling the fossil represented in their thirty-first plate, figure 1, the authors of the "Fossil Flora," proceed to state, that the convex specimen "has detached itself from the roof, which none of the before-cited instances did. This exhibits the same wrinkled appearance, with indistinct circular spots, as the under side, described vol. i.

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* Transactions of the American Philosophical Society, N. S. Vol. i. 1818.

† "Fossil Flora," vol. ii. Preface, p. xiii.

page 104; it has nine arms, five of which subdivide into two branches, at about eighteen inches from the centre of the fossil, and one at three feet; in this, as in the other instance, they are all broken off short." * But the branches—what connection have they with the specimen upon which they rest? This is a question that does not appear to have been sufficiently attended to when drawing up the description just quoted. From the remark elsewhere made, that Stigmaria was "of a yielding fleshy substance, with numerous arms, proceeding on all sides from a central dome,"† one would be induced to suppose that the "arms" of the convex specimen grew out from the margins of the "central portion." I cannot agree to this, because the specimen affords no evidence in support of it: on the contrary, nothing is more easy to prove, than, that the arms or branches had *no other connection with the specimen but that of superposition.* The proving of this may be effected by simply removing the branches, when it will be seen that the markings on the channels are perfectly continuous with the wrinkles on the central portion: were it as is generally supposed, the wrinkled part would here and there display a fractured surface, arising from the breaking off of the branches.

From what has just been said, it is evident that the markings on the central portion of the convex specimen have been produced in the same manner as the scars and wrinkles on the channels; or, in other words, that they are merely impressions which have been derived from a superimposed body: in short, it follows, that this specimen is nothing more than an indurated mass of mud, precisely similar to what must have occupied the hollow or under surface of the fossil represented in plate 31, fig. 2, of the "Fossil Flora;" and, that the branches are portions of a Stigmaria which originally rested upon it, and which probably still remains fixed in the roof of the pit.

The explanation which has been given, there is every reason to suppose, would not have been required, but for the fact, that the branches have undergone so much compression, especially at their upper extremity, as to produce the appearance as if they had grown out from the sides of the specimen, and as if the markings on their upper surface were continuous with the wrinkles on the crown. I am fully persuaded, it is entirely through this deceptive appearance that Lindley and Hutton have been led into the belief that *Stigmaria* had a dome-shaped centre, from which numerous arms proceeded on all sides. Agassiz also appears to have been misled in the same manner.‡

Another point remains to be disposed of. From the "ideal vertical section," which is given in plate 31, figure 2, of the "Fossil Flora," it might be concluded that *Stigmaria* had a rounded or convexly formed upper surface The untenableness of this conclusion will, however, be manifest from the fact, that, up to the present time—although I have now examined several specimens—I have not fallen in with one which exhibits the upper surface otherwise than truncated: further, when the centre of a Stigmaria has fallen from the roof, a portion in the shape of a stem, and answering to the truncated surface, is often seen remaining in the roof, and passing upwards.*

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* "Fossil Flora," vol. ii. preface, pages xiii. and xiv. † Ibid. p. xv.

‡ Translation of the "Bridgewater Treatise."

The disposal of these two points (and they have, I am decidedly of opinion, been the greatest hinderances to the working out of the true character of this fossil), not only proves, that the prevailing opinion regarding the form of *Stigmaria* is erroneous, but it leads to the inference, that what has hitherto been looked upon as the centre of this fossil, is in the form of a root stock deprived of its stem.

All the specimens of *Stigmaria* which have been seen in a perfect condition, or nearly so, have their branches running out in the manner of wide-spreading roots: this will be sufficiently evident by consulting figure 1, plate 31, of the "Fossil Flora." Besides the specimen just referred to, several others are still to be seen fixed in the roofs of several pits in this district. At Felling, a very large specimen occupies the roof of one of the galleries: it is impossible to ascertain its size, as it passes into an unworked part of the mine: some of the branches, at a little distance from their commencement, measure eighteen inches across in the compressed state. An idea may be formed of the dimensions which some Stigmarias attain, from the statement communicated to me by the head wasteman of Felling pit, that he has traced the impression of a single branch for full fifty feet, without finding its terminations. I am quite disposed to credit this statement, for I myself have measured an impression of this fossil in the roof of Jarrow pit, and succeeded in following it for thirty feet, until it disappeared in a part where the workmen had not carried on their operations. And, very lately, I have obtained for the Newcastle Museum, portions of three different specimens, equally confirmatory of the immense size which some Stigmarias attain. The first is a branch fifteen feet in length, rather flexuose, and remarkably uniform in thickness, which is four inches. The second differs from the last in being singularly yet gracefully tortuous, and in diminishing at one end to a mere film; the opposite end appears to have been joined to the main body of the fossil; or, what is more probable, this specimen may have been one of the off-sets of a divided branch; it is eleven feet long, and five inches in thickness, at the largest end. The third consists of three furcated branches, which have been broken off from the central stock; the two off-sets of one of these furcated branches are respectively seven and two feet long, and both are truncated. This specimen is by far the finest that has ever been procured in this district; and it appears, from its superficial characters, to be specifically distinct from *Stigmaria ficoides*. These characters will be reverted to in another part of this paper.

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* In general, these perpendicular stems are so polished or slickensided, that it is impossible to identify them with any known fossil: and nothing is more common than to see the surface of a so-called centre of Stigmaria as smooth as glass. The last is called a "kettle bottom" by the miners. I would suggest that this phenomenon has been produced by the fossil offering some resistance to the enormous compression into which the surroundingg rock has been subjected.

All the specimens of *Stigmaria* which have been described as occurring in the pits of this district *are in the roof*, and consequently only their under side is exhibited. There is a very large decorticated specimen, however, in the Ouse Burn, about two miles from Newcastle, having its upper side exposed. When discovered, the part which answers to the stock or centre was visible; and from all that I can ascertain, both from the fragments still remaining, and a sketch made at the time by Mr Albany Hancock, who was the first to make the specimen known, this part was broken in such a manner as to induce the supposition that a stem had been originally attached to it. The only external character which was displayed on the surface of so much of the branches as was visible at the time of the discovery of this fossil, consisted of a number of rude flutings, which character was the cause of some supposing it to be the branched apex of a Sigillaria: it was further supposed, that the stem had been destroyed or removed; and, that the apex, by some means or other, had been overturned, and afterwards covered up with sediment—now an argillaceous sandstone. The complete absence of scars was also appealed to as confirmatory of its belonging to the last-named genus, since decorticated specimens of *Sigillaria* are occasionally to be met with, divested of this character.

This allusion to the Ouse Burn fossil, makes it necessary for me to mention, that, on May the 17th, 1841, and shortly after the Newcastle Museum had become possessed of the earliest received North Biddick Sigillaria, I read, at a meeting of the Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne, a paper which gave an account of the last specimen, and which adverted to a character occurring on the under side of its base, " apparently leading to the conclusion, that *Stigmaria* is the root of *Sigillaria*."

The reading of this paper having brought before Mr Hutton, who was present, a question which had often occurred to him, he kindly drew my attention to the Ouse Burn fossil, as likely to assist me in my future researches. A few days afterwards, we examined this fossil, when I became convinced that it was not the branched apex of a Sigillaria, but the central stock of a Stigmaria. Mr Hutton having some doubts on this point, but being anxious to have it settled, he empowered me to employ some workmen to lay bare the branches; and, with his accustomed liberality, he went to considerable expense in the prosecution of this object. Two of the branches were exposed for upwards of six feet, and both were seen to divide, and to dip into the rock at an angle of 40 degrees to the line of stratification: the flutings were observed to become more and more indistinct as they passed downwards; but still no scars were visible. This was the result of our first inspection. I confess, that at this stage of the inquiry, the opinion I had formed was almost forsaken.

However, the idea occurred to me to examine the rock which the workmen had removed, especially those portions which had been in immediate contact with the fossil: this immediately led to the discovery of the cuticle adhering to those portions, in the condition of a rotten carbonaceous layer, which, on being removed, shewed the wrinkles and scars of *Stigmaria*. Nor was this all; the appendages were seen to be attached to wherever the scars were visible, and to penetrate the rock in regular directions. Thus, the evidences as to the fossil being a Stigmaria were conclusive; and it may be added, that the running out of its branches, in the manner of wide-spreading roots, was placed beyond dispute.

The well-known appendages, so often seen attached to the branches of *Stigmaria*, have, in general, been looked upon as leaves. They are nearly always flattened; occasionally, however, they are observed to be round, or, rather, vermicular—thus reminding one of the fibrils of the yellow water lily (*Nuphar lutea*); and there is little doubt, from the form of the scars which they leave when detached, that they were of this form originally. A beautiful and instructive example, shewing the appendages completely vermicular, has been figured by Sternberg.*

Steinhauer says, he found traces of these appendages proceeding from a branch " in every direction, to the distance of 20 feet,"—a statement which Lindley and Hutton think has originated through some error of observation, since they have never been able to trace them to a distance of more than 3 feet; but, it would appear, from the researches of Mr Logan, in the coal-mines of South Wales, that the length which Steinhauer gives to the appendages is not at all exaggerated,—the former having " traced them in a vertical direction, 7 or 8 feet from the stem, and more than 20 feet horizontally."† Up to the present time, I have never seen them exceed 18 inches.

Artis in his " Antediluvian Phytology," represents the appendages as forked.‡ I cannot say whether this character has been observed by any other observer. Once I saw an appearance of the kind; but still I cannot urge it with any degree of confidence.

One of the most important circumstances connected with the appendages, is the regular manner in which they are arranged around the branch to which they are attached. Steinhauer appeals to this circumstance as a proof that *Stigmaria* was a root which grew at the bottom of fresh water lakes, and which "shot out its fibres in every direction through the then yielding mud."§ The following extract from the " Fossil Flora" is to the same effect:—" The leaves also, which thickly surrounded the arms, could not, under any circumstances, even supposing them to have been hard woody spines (which assuredly they were not), have taken the direction in which we now find them, proceeding from the stem on all sides at right angles to its axis, and penetrating the shale, even perpendicularly up and down to the extent of two or three feet at least; had the plant been floated, the leaves, on the contrary, must of necessity have been pressed upon the arms, surrounding which we should have found their remains in confused masses, and spread out irregularly by their side, in the plane of the surface on which the plant had finally reposed; none of this, however, takes place; but, on the contrary, when the shale is split, so as to expose the surface of the fossil, the leaves are seen proceeding with the greatest regularity, each from its separate tubercle—those only being distinct in the length and breadth, which, when in a growing state, had been shot out in the plane which is now the cleavage (line of deposition) of the shale. From all these circumstances, we are compelled to conclude that these *Stigmariæ* were not floated from a distance, but that, on the contrary, they grew on the spots where we now find their remains, in the soft mud, most likely of still and shallow water."* Dr Buckland, however, has arrived at a different conclusion. He says, "All these are conditions which a plant habitually floating, with the leaves distended in every direction, would not cease to maintain when drifted to the bottom of an estuary, and there gradually surrounded by sediments of mud and silt."† These are conflicting inferences, let us endeavour to ascertain which is the true one.

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* " Flore du Monde Primitif," Parts 5 and 6, Tab. xv., fig. 4.

† Buckland's Anniversary Address to the Geological Society, delivered in 1841, page 34.

‡ Plate III.

§ " Transactions of the American Philosophical Society," N. S., Vol. i., p 273.

The data which the "intelligent observer," Steinhauer, has adduced, in support of his opinion that *Stigmaria* "shot out its fibres in every direction through the then yielding mud," may be received without the least hesitation, since they have been observed by too many witnesses to be doubted for a single moment. The diagram which Lindley and Hutton have given to represent the appendages "proceeding from the stem on all sides at right angles to its axis, and penetrating the shale even perpendicularly up and down," may occasionally be seen represented on hand specimens. I have lately obtained one of this kind, which is now in the Newcastle Museum: it is a forked branch resting upon a matrix of shale; the last is made up of countless laminæ, which there is little doubt were deposited horizontally: both the forks *cut* the laminæ at an angle of twenty degrees. As this specimen is deprived of the superimposed portion of its matrix, it is of course deficient in enabling us to trace the direction of the appendages which proceeded from its upper surface; but it furnishes us with positive evidence respecting the direction of those which passed from its sides and under surface.

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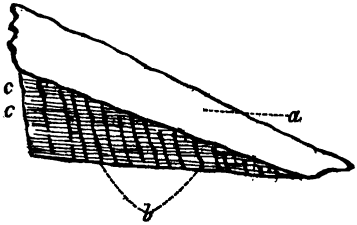
* "Fossil Flora," Vol. ii, Preface, p. xvi.

† "Bridgewater Treatise," Vol. i., p. 477, foot-note.

I have endeavoured in the annexed sketch, to represent one of the



forks or branches (*a*) of this specimen, with its appendages (*b*) penetrating the laminæ (*c c*.) The lateral appendages are not represented; but it may be stated, that they run out in the same plane as that of the branch—thus agreeing with the diagram before mentioned. With respect to the appendages which proceeded from the upper surface, it may, I think, be safely concluded from the concurrent testimony of Steinhauer, Hutton, Logan, and others, as to the direction of similarly situated appendages found on other specimens of *Stigmaria*, when *in situ*, that they passed upwards, and probably formed the same angle with the branch as is made by their analogues on the under surface.

Respecting the question as to how have the appendages become so regularly arranged in their matrix, which, judging from the perfect parallelism of its constituent laminæ, shews that no obstruction prevailed during its deposition,—there can, I think, be only one opinion, which is, that the matrix was deposited in the first instance, and, that the appendages penetrated the latter, when it was in a yielding condition. To bring forward arguments in support of this opinion is clearly superfluous, since it is demonstrated by the simple fact of the laminæ of deposition being so *regularly arranged*. Had the matrix been deposited upon or around the appendages, it is difficult to conceive any thing else than that the laminæ would have been *exceedingly irregular*. But, admitting for a moment that *Stigmaria* was a floating plant, under what circumstances, I would ask, have the inferior appendages been able to preserve a downward, the lateral ones a horizontal, and the superior ones an upright direction? The only way to account for these facts, is to suppose that the appendages were of the nature of spines, and strong enough to have retained their original direction, had the plant become stranded or covered up with silt; but against such a supposition may be urged the succulent nature of the appendages, as proved by their being composed, with the exception of a central bundle of spiral vessels, of a thin walled cellular tissue,* and by their occasionally being found twisted and matted together in the greatest confusion. Thus, no circumstance can be conceived to render, even probable, the opinion that *Stigmaria* was a floating plant; but, on the contrary, every thing evinces the soundness of Steinhauer's view, that its appendages did in reality penetrate their matrix in the same way as root fibrils.

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* Goeppart " Uber die Stigmaria eine neue Familie der Vorweltlichen Flora," in Karsten and Von Dechen's Archives, xiv,, 1840.

Having proceeded thus far with our inquiry, we may for a moment stop to give a brief recapitulation of the results that have now been arrived at, as to the characters of *Stigmaria*, and also, to consider the general corollary deducible therefrom. These results are, 1*st*, that *Stigmaria* possessed a centre, in the form of a root-stock deprived of its stem; 2*d*, that its centre was furnished with branches, which ran out in the manner of wide-spreading roots; and, 3*d*, that its branches were provided with appendages, which penetrated the matrix in which they are imbedded in the same way as root fibrils. All these characters, there can be no hesitation in saying, amount to a complete demonstration that *Stigmaria* fulfilled the purpose of a root.

We have now to consider the plant to which this root belonged.

The principal vegetable fossils of the coal-measures in the shape of stems, are included in the following list:—*Megaphyton*, *Ulodendron*, *Lepidodendron*, *Auracaria* (?), some other Conifers, *Caulopteris*, *Anabathra*, and *Sigillaria*. With one exception, the whole of these may be readily disposed of. *Megaphyton* is too rare to have belonged to a fossil so abundant as *Stigmaria*; *Caulopteris* is generally admitted to belong to a different group of plants—the Vascular Cryptogams; *Lepidodendron* is without the ligneous cylinder of *Stigmaria*; *Ulodendron* is probably in the same predicament; *Auracaria* (?) and the other Conifers have the walls of their woody tissue furnished with discs instead of stripes, as in *Stigmaria*; and *Anabathra*, although it offers a considerable approximation to *Stigmaria* in the character of its ligneous tissue, its extreme scarcity places it in the same category as *Megaphyton*. *Sigillaria* only remains to be considered, and this may be done by giving a synopsis of parallelisms between it and *Stigmaria* in the first instance, and then to enter somewhat into detail respecting some other points of agreement.

Sigillaria	*Stigmaria*
is rooted in beds of the coal-formation, as proved by the Dixon-fold, Killingworth, and North Biddick specimens:	vegetated in beds of the coal-formation;
possesses enormous roots, as shewn by the Dixon-fold fossils:	is of enormous size:
is one of the most abundant stems of the coal-formation:	*abounds* in the coal-formation:
possesses a ligneous cylinder, the tissue of which is marked with stripes, and arranged in radiating series:	possesses a ligneous cylinder, the tissue of which is marked with stripes, and arranged in radiating series:
is furnished with forked root branches, as shewn by the Dixon-fold specimens,	is furnished with forked branches,

These points of agreement between the stem *Sigillaria* and the root *Stigmaria* cannot but give rise to the suspicion, that they are the parts so named of one and the same plant.

The examination of the Ouse Burn fossil brought to light a character which, until then, was not generally known as belonging to *Stigmaria*. I allude to the rude flutings. The Newcastle Museum contains several specimens displaying the same character. One of these shews the external surface of the cuticle ornamented with a number of scars; but the latter are only seen on the median part of the ribs—the furrows being, like those of *Sigillaria,* entirely without them: the wide difference between this specimen, and those usually met with, led some to suppose that it belonged to an undescribed genus. It was its fluted character that caused me to maintain that the Ouse Burn fossil was a Stigmaria.

It will be recollected, when the North Biddick stems were described, that the ribs at their base were stated to be nearly obsolete. Now, the rude flutings of *Stigmaria* have very much the aspect of these obsolete ribs—the only difference being, that the former are not so regular; the resemblance, however, is so striking as, of itself, to suggest the opinion that *Stigmaria* is nothing more than the root of *Sigillaria;* but, considering that the rude flutings, and the nearly obsolete ribs are on those parts where it may be supposed the two fossils were joined, and consequently where the former passed into the latter, it would appear that this opinion is so far rendered extremely probable. If the Dixon-fold specimens have the surface of the upper portion of their roots rudely fluted, it would follow that at this stage of our inquiries the question was completely settled. I have not seen the Dixon-fold Sigillarias myself, but Mr Morris, who has, and who has also examined the fluted Stigmarias in the Newcastle Museum, has assured me, that the flutings on the base of some of the former closely resemble the same character on the latter. The account which Mr Hawkshaw has published of the Dixon-fold specimens would even incline one to rest satisfied on this point; for, he says, near the root of one (No. 5.) a coaly envelope remains, about three-fourths of an inch in thickness, having "its surface marked slightly with longitudinal furrows, but they are very irregular in distance and direction."*

The two specimens of *Sigillaria* from North Biddick, as they now stand in the Newcastle Museum, shew no indications of root-branches; but on some portions which formed the root-stock of one, and which it has been deemed necessary to preserve separately, there is a decided appearance, not only of marginal branches, but also of others originating inwardly to the latter. The branches are unfortunately broken off, or truncated, almost at their commencement; what remains is, however, of considerable value in our inquiries.

It is well known that the cuticle of *Stigmaria* is more or less wrinkled, and that the wrinkles run in flexuose lines. With the exception of those parts from which the root branches have been broken off, the whole of the under surface of the root-stock mentioned in the last paragraph is crowded with flexuose wrinkles, which in no respect differ from those of *Stigmaria*.* There are certainly none of the scars so commonly seen associated with the wrinkles of Stigmaria; but their absence on the under surface of the root-stock of the North Biddick Sigillaria will be readily explained by the fact, that the corresponding part of the former is equally divested of scars. As bearing upon the last point, it requires to be mentioned, that the Stigmaria, figured in Pl. 31, fig. 2. of the "Fossil Flora," (Vol. i.) is said to have had the whole of its under surface "distinctly covered with wrinkles, which, when attentively examined, are seen to be caused by depressed semicircular spots, compactly arranged in a spiral manner, in the centre of which is a roundish scar, to which a fine coaly matter usually adheres:" further, the convex specimen, described in the preface of the second volume, is stated to exhibit "the same wrinkled appearance, with indistinct circular spots," as the last. As I have only seen the dome-shaped specimen, I can, of course, only speak with reference to it, and I shall do so with the view of preventing the supposition arising, that the "indistinct circular spots" are such as to shew, that the convexity of the last fossil, and the concavity of the North Biddick Sigillaria, exhibit totally different characters. It is necessary, however, to remark, in the first instance, that as the convex specimen is merely an impression of the outer surface of the cuticle of a root, it is obvious, a knowledge of the character which this surface displayed would materially assist us in our immediate object. Influenced by this consideration, I took a plaster cast of the crown of the specimen; by this means I succeeded in obtaining an exact copy of the surface of the cuticle, which copy displayed a character strikingly resembling—though on a smaller scale—the elongated lozenge-shaped rifts characteristic of the bark of some of our forest trees, especially the ash *(Fraxinus excelsior.)*

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* Transactions of the Geological Society. Second Series, Part VI. p. 174.

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* I discovered the wrinkles on the under surface of the root of the North Biddick *Sigillaria* a few days after it was deposited in the Newcastle Museum; at the same time, I was struck with their resemblance to those of *Stigmaria*: I was thus led to suspect that the one plant might be the root of the other: the highly problematical nature of the last fossil had also considerable share in eliciting this suspicion. I was then preparing a short paper descriptive of the North Biddick fossil, to be read at the next meeting of the Society; so I resolved on making this the vehicle of my suspicion, and to embody in it all the arguments I was master of in support of the same. This paper was read May 17. 1841. On referring to it at the present moment, I find, besides the above-mentioned point of agreement, that the fact of the appendages of *Stigmaria* having penetrated their matrix was strongly insisted upon: there were also some vague allusions to the convex specimen in the Museum, answering to the under surface or hollow of a root: nothing was said of the analogy between the rude flutings of *Stigmaria* and those on the base of *Sigillaria*, although, at that time, I was acquainted with this character occurring on the former; indeed, as remarked in the text, the rude flutings formed one of my chief arguments for maintaining that the Ouse Burn fossil was a Stigmaria. When I became acquainted with the last specimen, I was in possession of clearer views respecting the convex fossil, and I had also read the accounts of the Killingworth specimen; these circumstances, and the fluted character of *Stigmaria*, strongly fortified me in my opinion. Shortly after it was settled that the Ouse Burn fossil was a Stigmaria, Mr Hutton very kindly lent me his copy of Brongniart's "Observations on the internal structure of *Sigillaria elegans*, &c.," and I was delighted to find that *Sigillaria* and *Stigmaria* agreed so closely in their internal structure, and that Brongniart himself, from this circumstance, and the creeping habit of the latter, had been led to suspect that the one was nothing more than the root of the former. From that time to May 1842, I was more or less engaged in working out this question; and at the date just mentioned, I commenced the reading of a paper (then in a nearly finished state), substantially the same as the one now in course of publication. In order, that every one should have his due share of credit who has anticipated the view which is now being advocated, the name of Professor Lindley must not be overlooked; in the article "Coal Plants," of the Penny Cyclopædia, written in 1386 or 1837, he asks, "is it quite impossible that Sigillarias and Stigmarias are both the same thing; the former being the stem, the latter the roots?" after which, he very briefly refers to the root branch, and base of the stem of the Killingworth fossil, figured in Pl. 54, "Fossil Flora" so, as the reader might compare them with the Stigmaria represented in Pl. 31, fig. 2 (Qu. fig. 1?) of the same work (April 1844.)

As to the scars—that is, such as have resulted from the falling off of the fibrils, not a single one was visible; they were, in fact, only seen where the branches had left their impression on the channels. The absence of the rifted character on the under surface of the root of the North Biddick Sigillaria, and its presence on the same part of the root which produced the convex specimen, seems to be due simply to a difference of age, and to a rifting of the cuticle of the last example, through an increase of the tissues which this cuticle originally enclosed; at least, such are the apparent causes of the disparity between the bark of old and young individuals of the ash; and there appears to be no reason why the same causes have not operated in producing the like difference noticed in the fossils.

There yet remains another point to be mentioned in favour of *Stigmaria* being the root of *Sigillaria*. As previously remarked, the character which has just been concluded is only to be seen on the root-stock of one of the North Biddick specimens. The root-stock of the other has also been preserved separately; but instead of its under surface displaying any wrinkles, there are visible two strongly-marked furrows, which cross each other in the centre, and at right angles, and which disappear at the margin: this difference is probably due to both fossils being preserved under different circumstances. I have observed similar furrows on the under side of a root-stock of a stem—apparently of a Sigillaria, belonging to Wm. J. Charlton, Esq. of Hesleyside. Now, on examining the crown of the dome-shaped specimen,—which, it must be remembered, has been proved to be merely the indurated mud which occupied the hollow or under surface of a root,—there are actually displayed two strongly-marked *ridges*, crossing each other precisely under the same circumstances as the *furrows* of the aforementioned Sigillarias. It will doubtlessly occur to many, that these furrows are analogous to those usually seen on the under surface of the branches of *Stigmaria*, and that they have been produced in the same manner,—that is, by the settling down of the ligneo-vascular cylinder after the destruction of the enveloping and less enduring cellular tissue. According to this view, the intersecting furrows on the fossil root-stocks, will have resulted from the ligneo-vascular cylinder of the stem dividing itself into four parts; and these divisions striking off into the root-branches, at right angles to each other: a circumstance of this kind, it is conceived, would give rise to that appearance of crossing each other which the furrows present.

I cannot conclude the present inquiry more appropriately than by quoting a portion of Mr N. Wood's description of the Killingworth Sigillaria,—observing, in the first place, that the specimen, which appears to have been about 10 feet in height, was discovered with several others, in a nearly vertical position, and evidently rooted in a thin bed of argillo-bituminous shale, overlying a workable seam of coal: "The lower part or base of the tree was about two feet in diameter, flattening out considerably at the bottom; this part was so much broken that it could not be procured; but the bed of it, with the roots proceeding from it, was most clearly seen *in situ*. The roots could be traced for about four feet from the stem, penetrating the shale; but the compact nature of the shale prevented us from obtaining specimens, when the thickness of the roots diminished; but they were seen running into the shale composed of the same kind of sandstone, though a little more indurated, until they were less than an inch thick. * * * * The roots were not numerous, but ran into the shale quite parallel with the inclination of the beds, and spread out from all the different sides of the fossil."* To this account, it is necessary to add, that the specimens themselves of the divided root branch, and the stem, which are figured in Mr Wood's paper, are at present in the Newcastle Museum: the root-branch is decidedly a Stigmaria, and the stem *appears* to be a Sigillaria.

Thus, bearing in mind that *Stigmaria* has been proved to have been a root; and, considering the tendency of the evidences latterly adduced, it now seems to be all but demonstrated, that this fossil was the root of *Sigillaria*.

(*To be concluded in our January Number.*)

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* Transactions of the Natural History Society of Northumberland, Durham, and Newcastle-upon-Tyne, Vol. i., p. 210.

On the Action of Yellow Light in producing the Green Colour, and Indigo Light the Movements of Plants. By D. P. Gardner, M.D., Cor. Memb. Lyc. Nat. Hist. New York.

1. The object of this paper is to prove the existence of different properties in the rays of the spectrum, in their action on vegetables; and more especially to shew that the rays which produce the green colour of plants, are altogether dissimilar from those which influence their movements towards light, the colour being developed by the less refrangible rays, and chiefly by the *yellow;* whereas, the motion is influenced by indigo light. The discussion of the subject will be divided under three heads:

1. On the production of chlorophyl by yellow light.
2. On the movements of plants towards indigo light.
3. Some application of these facts to vegetable physiology.

1. *On the production of chlorophyl by yellow light.*

(2.) It is a fundamental fact in botany, that light is necessary to the formation of chlorophyl. Von Humboldt adduced certain exceptions to this law, in the case of plants found in the mines of Freyberg, and, with Senebier, ascribed the green matter to the action of hydrogen gas. But the experiments of the latter failed in the hands of De Candolle, and a series instituted by myself, and conducted with great care, were equally unsuccessful. On the other hand, Humboldt succeeded in greening a plant of *Lepidium sativum,* raised in darkness, by the light of two lamps, and De Candolle obtained the same result with six Argand lamps.

(3.) The investigation has been subsequently confined to the name of the ray which produces chlorophyl.* Formerly it was tacitly admitted, on the authority of Senebier, that the *chemical*, or blue ray, was most active. Professor Morren (Annal. des Scien. Nat., Oct. 1832) ascribed it to the luminous colours, more especially the rays which had passed through bright yellow and orange glasses. Dr Daubeny (Phil. Trans. 1836) in his valuable researches, arrived at the same conclusion. The next investigator, Dr Draper (Jour. Franklin Inst. 1837) obtained better results in yellow than blue light. Mr Hunt, in 1840 (Lond. and Edin. Phil. Mag. April), resumed the question, and published the most decided results (p. 272), to the effect, that *blue light alone* causes the green colour of plants, and that the yellow and red rays, "*destroy the vital principle in the seed.*" In 1841, he was one of a committee appointed by the British Association, to report on this subject, and in a subsequent conversation at the late meeting of that body, has repeated his statements. Being the last writer, his results have given a prominence to the doctrine, that chlorophyl is produced by the blue rays, so as to mislead Professor Johnston in his agricultural lectures, and Professor Graham (Chemistry, p. 1013).

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* Chlorophyl, the green matter of leaves. It is insoluble in water, but soluble in ether and alcohol. The ultimate analysis has not been made; but chemists agree that it is of the nature of wax. The yellow colour of autumnal foliage is due to a similar yellow wax, called Xanthophyl, supposed to be produced by the action of frost on the former substance.

(4.) In September 1840, I repeated Mr Hunt's experiments in Virginia, and obtained dissimilar results. A known number of turnip seeds were sown, and every grain germinated in the yellow and red rays. The greenest plants were found in yellow light. Every condition was favourable, and the results well characterised, but my reason for deferring the publication arose from a conviction that the use of solutions and coloured glasses was objectionable, and that *no perfect results could be obtained except with the spectrum.* Plants exposed to light which has permeated cobalt glass, are not placed in blue rays, but in red, yellow, green, blue, indigo, and violet, in proportions differing with the tone of colour, and thickness of the material. The effect may therefore be produced by any of these rays, or by their peculiar combination. (See Sir J. F. W. Herschel's paper in the Philosophical Transactions for 1840, p. 24, on "The combined action of rays of different degrees of refrangibility.")

(5.) I shall not attempt to explain the discrepancy between my results and those of Mr Hunt, for I do not esteem researches such as all the foregoing, made with coloured media, of any value in this branch of vegetable physiology. It is well to remark, however, that in treating of the germination of cress seed behind the blue, green, yellow, and red media, he states, " that the earth continued *damp* under the *green* and *blue fluids*, whereas it *rapidly dried* under the *yellow* and *red.*" (p. 271). This difference would by most persons have been considered sufficient to retard or " destroy" ? germination.

(6.) Other engagements in 1842 interfered with my design of examining this question with the spectrum; and it was not until July 1843, that such arrangements were made as are necessary to the prosecution of the subject.

(7.) *The apparatus.* A beam of the sun's light was directed by a heliostat placed outside my window, along a square tube of wood, passing through the shutter. The inner extremity of the tube was closed, and contained near its end a flint glass equilateral prism, one inch on the side and six inches long, with the axis adjusted perpendicularly. The dispersed light passed into the chamber through an aperture in the side of the tube. All that portion of the beam which exceeded the breadth of the prism was cut off by a diaphragm. The object of these arrangements was to render the room dark. The experiments were performed in Virginia, in lat. 37° 10′ N., and continued from July 6 to October 1, during a season of unusual brilliancy and temperature.

(8.) *Arrangements for the experiments.* Seedlings of turnips, radish, mustard, pease, several varieties of beans, peas, and the following transplanted specimens, were used, *Solanum nigrum, S. Virginianum; Plantago major, P. minor; Polygonum hydropiper; Chenopodium rubrum; Rumex obtusifolius.* They were placed in boxes with partitions, or planted in jars, and grew in darkness until ready for experiment, so that they acquired a yellow colour. The number of plants exposed to each ray averaged one hundred, when the smaller seeds were used, and the result indicated was obtained by a comparison of the whole. The age of seedlings is a matter of moment; those which are young, and from one inch to one inch and three-fourths high, in the case of turnips, were most sensitive; indeed these plants were found to give the best results, and were used almost exclusively after the first month.

The spectrum was allowed to fall on the specimens at a distance of fifteen feet from the prism, and undecomposed light shut out by screens. Each ray acted in a separate compartment, unless otherwise stated.

(9.) The following extract of an experiment, will shew some farther details:—

"*August* 13.—Five jars, containing each about one hundred turnip seedlings, were placed respectively in the orange, yellow, blue, indigo, and violet rays, at 9 h. A.M. Day bright; temperature in shade at noon 80° Fah., in the sun 95°. Duration of sunshine 6½ hours. Result at 3½ P.M. The third column of the table shews the altitude of the plants at the commencement of the observation:

Jar.	Light.	Height at 9 Hours.	Result.	Order.
1	Orange.	1 inch.	Green.	2
2	Yellow.	1 "	Full Green.	1
3	Blue.	1¼ "	Slight Olive.	[*]
4	Indigo.	1 "	Yellow.	0
5	Violet.	1¼ "	Yellow.	0

"*August* 14.—The same plants, with the addition of a fresh crop (6) in the green ray. Exposure from 9 A.M. to 3 P.M., or six hours sunshine. Temperature in shade at noon 85° Fah. and 105° in the sun. Result at 3 P.M.

Jar.	Light.	Height at 9 Hours.	Result.	Order.
1	Orange.	2¼ inches.	Full Green.	2
2	Yellow.	2¼ "	Perfect Green.	1
3	Blue.	3 "	Slight Green.	4
4	Indigo.	3 "	Yellow.	0
5	Violet.	3½ "	Yellow.	0
6	Green.	1 inch.	Fair Green.	3

The leaves of 1 and 2 were developed. *Experiment* concluded after 30 hours, of which 12½ were sunshine, and 17½ darkness. The greater altitude of the plants in the indigo and violet rays, a fact discovered by Morren, is due probably to the slowness of exhalation by vegetables in those colours, an effect not of light, but of heat. In this observation, no result whatsoever was produced on the original yellow colour of the seedlings in the indigo and violet rays.

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[*] The fifth column contains a comparative estimate of the depth of colour, assuming unity of the highest value; on this scale the plant in blue light did not become green, and the value is negative, but there was a visible alteration designated olive, and indicating the tint which vegetables assume in passing from the yellow colour of darkness to green.

(10). The ensuing table contains the comparable points of six similar experiments. The 1st column gives the number of the observation; the 2d, the plants used; the 3d, the number of hours of sunshine; the 4th, the whole duration of the experiment; and from the 5th to the 13th column, the rays of the spectrum; the figures in the last spaces indicate only the order of colour in the particular observation. The sign of minus is introduced whenever the effect of the ray was not tested, or the result was defective.

Table, *shewing* the Active and Inactive *Rays of the Spectrum, in producing the Green Colour of Plants.*

Experiments.	Plants.	Hours of Light.	Total Time.	Active Rays.				Inactive Rays.			
				Red.	Orange.	Yellow.	Green.	Blue.	Indigo.	Violet.	Lavender.
1	Turnips.	22	109	4	2	1	3	0	0	0	0
2	Beans, &c.	14	95	—	2	1	3	0	—	—	—
3	Turnips, &c.	8	69	4	2	1	3	—	—	—	—
4	Turnips.	23	101	—	—	—	1	0	0	0	0
5	Turnips.	17·5	52	—	2	1	3	4	0	0	0
6	Turnips.	5·5	6	4	2	1	3	0	0	0	0

In experiment 5, the blue ray produced a green colour, but the usual effect was a light olive. The indigo, violet, and lavender portions were always inactive, although several observations were continued until the plants faded.

(11.) Under favourable circumstances it requires a long exposure to develope chlorophyl. The shortest period I witnessed was in a crop of turnip seedlings, which required two hours in the centre of the yellow rays, but frequently six or more hours were necessary. In the full sunshine of Virginia, it requires more than one hour to produce the same effect.

The colour acquired is not fugitive. It has been observed scarcely changed after seventy-two hours' darkness, in turnips, and seven days in beans. Plants from the field preserve their colour sometimes for weeks, but finally become yellow.

(12.) The fact established by these experiments is, that the less refrangible rays are most active in producing the green colour of plants. It is not stated that the blue, &c. rays will not effect this change in time, but that they are remarkably inactive.

(13.) *The maximum action is in yellow light.* For the purpose of obtaining a measure of the comparative activity of each ray, the following experiment was made. The spectrum of a circular beam of light, three-fourths of an inch in diameter, was received upon a double convex lens of three feet focus, placed near the prism. The dispersed rays passed through a chink of one-fourth of an inch, into a camera, and each fell into a separate compartment containing a few turnip seedlings, situated near the focus of the ray. The place of the extreme red and central yellow rays was determined through cobalt-glass, and the whole spectrum divided into the spaces given by Fraunhofer, for the width of each colour. The arrangements being carefully adjusted, the plants were examined at intervals, by allowing a little diffused light to fall upon them, and excluding the spectrum; in this way the number of hours was obtained in which a given ray produced a certain effect. The depth of green colour was estimated by carefully comparing the plants with a selected specimen; in this way I was assisted by a friend, whose eye is well skilled in distinguishing between shade, of colour.

(14.) The best result gave for the yellow 3½ hours, the orange 4½ hours, and the green ray 6 hours; the plants were selected from the centre of each group, and all the measures obtained on the same day, during uninterrupted sunshine. The experiment was continued until 17½ hours of sunlight had acted upon the seedlings in the blue space, which then acquired a tint, estimated at one half that of the test. In another observation, the indigo, violet, and lavender of Sir John Herschel produced no effect in 23 hours.

(15.) From those experiments, I conclude that *the centre of the yellow ray is the point of maximum effect in the pro-*

duction of chlorophyl; and that the action diminishes on either side, to the termination of *the mean red* and *blue.*

(16.) In this stage of the subject an interesting question suggests itself—Is the active agent *light?* some form of chemical ray? or heat?

To discover whether it was due to Tithonicity,* I placed a crop of turnip seedlings in a box, illuminated exclusively with light, which had traversed a solution of bichromate of potassa, sufficiently concentrated to absorb all tithonic rays. The plants became green in about 2½ hours, so as to indicate not only, that detithonised light was capable of producing the green matter, but of doing so with remarkable activity. *Hence, the formation of chlorophyl is not due to Tithonicity.*

Nor is heat the active principle, for the maxima of heat which has traversed flint-glass, do not correspond with the rays which produce the chief action on etiolated plants. *Chlorophyl is therefore produced by the imponderable light,* as distinguished from all other known agents found in the sunbeam.

2. *On the movements of plants towards indigo light.*

(17.) Among the most interesting phenomena of plants, is the apparent instinct of bending towards light. The character of the movement may be seen with ease, by exposing a crop of turnip seedlings near the light of an Argand lamp, provided with an opaque shade. If they be adjusted in such a manner as to leave the leaflets slightly above the lower margin of the shade, the whole will be found inclined forwards in two or four hours. It is this movement I propose to examine.

(18.) All erect plants obtained in darkness, when exposed to the solar spectrum, in distinct compartments, incline themselves forward towards the prism. It is, therefore, an effect which is produced in every variety of light; even obscure light can accomplish it; therefore, in researches on this subject every precaution must be taken to darken the place of experiment. The amount of bending frequently exceeds ninety degrees; and a movement of the fore extremity of the stem through one inch, to one inch and a half from the perpendicular, is not unusual in turnip seedlings.

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* See Dr Draper's paper in the Lond., Edin., and Dub. Phil. Mag. for Dec. 1842. Tithonicity is the name of an unponderable agent, supposed to differ from light, by being invisible, and from heat, by not being conducted by metals, and incapable of producing the expansion of bodies. From this term, tithonometer and tithonic rays are derived.

(19.) If the young plants be exposed to a spectrum, produced, as in art. 13, in a box without compartments, after a time they will be found inclined diagonally towards a common axis; those in the red, orange, yellow, and green, bending towards the indigo; and the plants of the violet and lavender spaces moving to meet them. When a larger spectrum of fourteen inches was used, and the seedlings exposed for five hours, they were so inclined as to suggest the appearance of a field of growing wheat, blown by two winds to a common point. If the experiment were sufficiently prolonged, some of the plants from either side of the spectrum interlocked in the direction of the axis.

(20.) *This axis is in the direction taken by Fraunhofer's indigo ray, in passing from the prism to the plants.* The seedlings growing in indigo light inclined directly along it; but those of the red and orange did not move towards the radiant in the prism, but along a diagonal, inclined in part towards the plants illuminated by the active rays, which were much nearer than the prism. The amount of this lateral inclination diminished as the plants were nearer the axis, so that those illuminated by blue, violet, and lavender, were little deflected from a line drawn from their place of growth to the radiant. Seedlings in the red, orange, and yellow rays, frequently bent to such an extent as to cause their summits to pass through the adjoining coloured space.

(21.) The secondary (lateral) inclination did not occur when the radiant was a reflected image of the spectrum, which was not allowed to fall on any of the plants. If the mirror reflected neither of the more refrangible rays, the plants appeared to be inclined to the light immediately before them.

(22.) These experiments satisfied me that the active force was in the indigo ray, and the intensity of the light necessary to produce deflection was extremely feeble, so that an amount inappreciable to the eye, which is an admirable measure of the intensity, but incapable of estimating the effect of quantities, would, after a lengthened exposure, cause considerable deflection. Indeed, the phenomenon is so little dependent on the brilliancy of light, that very little seems to be gained by concentrating the rays beyond a certain point. There is sufficient activity in each prismatic colour to produce bending, if enough of time be allowed. The movement is, therefore, a result depending upon the absorption of light.

(23.) As this is an entirely new subject, it is thought expedient to advance some further evidence concerning the position of the deflecting force. For this purpose the spectrum was allowed to fall upon a screen, perforated by two similar apertures, in such positions as to allow the red to pass through one, and the indigo through the other. Behind the screen a box was placed, containing four jars of turnip seedlings, arranged along a line occupying the centre between the intromitted rays. The light passed through the box without any reflexion, and was stifled by black cloth when it reached the further extremity. All the plants commenced bending in a short time, and in two hours the nearest group were inclined forwards 90°, and laterally 50°, towards the indigo aperture, the edges of which formed the radiant. In three hours the second crop exhibited the same movement; and so with the plants of the third and fourth jars. At the conclusion of the experiment, in six hours and a half, all were bent forward at about 90°, and each group inclined towards the indigo aperture in a direction indicated by drawing a straight line from the plants to the radiant. Not one plant inclined towards the red ray, although half the collection were at first nearer to it than to the more refrangible light.

With similar arrangements, the yellow, orange, and green rays were contrasted with the indigo, and always with the foregoing result. The time necessary to develope a satisfactory lateral inclination from the green rays, is greater than in the experiments made between the less refrangible rays and indigo.

(24.) The same results were obtained when the radiants were reflected images. The extent to which the influence of the active light is felt was frequently surprising; in some of the observations pea plants were four feet from the indigo, and within half an inch of the yellow, red, or orange radiant, notwithstanding which they inclined towards the indigo. In these researches, the mirror was so situated as to reflect no prismatic light upon the plants.

(25.) That no doubt may rest on the place of the soliciting force, another arrangement was used. The instrument figured by M. Pouillet (*Elemens de Phys.*, t. i. fig. 218) for examining the effect of combinations of rays of light in producing colour, was taken. Red rays were received on one mirror, and indigo on another; and the two so far inclined as to cause the rays to intermix at a place about three inches in advance of the instrument, and out of the incident beam. A jar of turnip seedlings was then placed so as to receive the compound light in its centre; the plants being illuminated in part by the red, indigo, and purple rays. In two hours the movements were considerable, and somewhat complex. Every plant lighted by the indigo rays was inclined directly to taht radiant. Those which received red light were bent to the central purple, and none to the red radiant. But many seedlings at first in the red, inclined themselves towards the purple, and after being fully illuminated thereby, commenced a lateral movement towards the indigo radiant; so that, at the close of the experiment, their stems exhibited two inclinations, the one in a vertical, and the other in a horizontal plane.

(26.) Plants raised in darkness, as well as those which were green, were used in the preceding observations; but the sensibility of the former greatly exceeds that of the latter. Indeed, plants that have been exposed to light for several days, become sluggish in their movements, and the phenomenon probably ceases in parts which are ligneous. In the seedlings submitted to examination the movements were found to take place in consequence of an action impressed upon the stem only; for the removal of the leaflets did not alter the result. A still more remarkable fact was discovered in all the cases observed—that after complete bending, plants erect themselves again when placed in darkness, at least in situations so dark as to appear entirely deprived of light. This effect is best seen in seedlings which have never been exposed to the direct rays of the sun; for, after full and lengthened exposure it diminishes almost to zero. The action of light in producing movement seems therefore to be transient; that is, it is not accompanied with a permanent change of structure in the stem.

(27.) From all the foregoing experiments, it is demonstrable, *that the force which constrains the movements of plants towards light, has its maximum in the indigo ray.*

(28.) But the solar beam contains a number of agents, one of which more especially developes itself in this part of the flint-glass spectrum acting upon argentine compounds with great effect. Dr Draper has discovered the existence of chemical action, distinct from the rays of light or heat throughout the spectrum, and terms the agent which produces it, Tithonicity. Is the bending of plants produced by the *tithonic rays? by heat?* or by *light?*

(29.) The investigation of these important problems has cost me much labour; but the following results will shew that a satisfactory solution has been attained.

A trough of plate-glass, containing persulphocyanide of iron, which has the property of absorbing the tithonic rays of the indigo space, and allowing indigo light to pass, was placed before a small aperture made in the side of a suitable box. The proper place for the hole was determined by receiving the analysed spectrum on a daguerre plate resting against the box. In a few minutes, two stains were observed, with an interval between them, corresponding to the place of the indigo light. The inactive space was marked on the wood, and a perforation made in its centre, without deranging the adjustment, so that the aperture continued to admit detethonised light. Plants placed in this box were bent in two hours, whilst a crop illuminated by indigo rays, which had not been transmitted through the solution, did not move with much more activity, although one crop was exposed to the maximum of the indigo tithonic rays, and the other placed in detithonised light.

(30.) Solution of bichromate of potash intercepts nearly all tithonic matter, but permits the free passage of luminous rays. A crop of turnip seedlings was introduced into a box and illuminated by the yellow rays of the spectrum, analysed by this solution. A daguerre plate was also introduced, to serve as a test of chemical action. In two hours and a half the plants were all equally bent, and the plate but slightly stained on one edge. A group of similar plants, exposed in the same place, without the solution, were inclined in a period of time not materially different. If the bending had been due to tithonicity, the seedlings should have moved towards the place where the silver was stained.

(31.) The tithonic activity of rays transmitted through the above solution, from an Argand lamp, is diminished to less than one two-hundredth part, as measured by Dr Draper's tithonometer.* But plants were bent in light from this source which had traversed the solution, in a period of time not much greater than that required in the full blaze of the lamp.

This result alone is abundantly sufficient to decide the question, and shew the total inactivity of the tithonic rays in producing these vegetable movements.

(32.) That the bending is not due to heat, appears from the following considerations: The action is greatest in those parts of the spectrum which give evidence of least heat: The axis is approached, on one side, by plants from the red, orange, yellow, and green, and by those from the violet and lavender on the other, which is a phenomenon altogether inexplicable, on the supposition that heat is the active agent.

Plants shut from the light of an argand lamp, by a plate of copper foil, do not incline to the warm metal.

Finally, the moonbeams, even without condensation, are capable of producing extensive bending in one or two hours. This result is conclusive of the question; for no trace of caloric can be found in the moon's light.

(33.) As far, therefore, as the presence of heat can be determined by thermoscopes, or the tithonic rays by argentine compounds, and the union of chlorine and hydrogen, we are justified in concluding that the movements of plants are effected by a totally different agent. *Light only remains in the spectrum, so far as we know; and to it, therefore, I refer the motions under consideration.*

(34.) This conclusion is of deep interest, inasmuch as it is

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* Tithonometer—an instrument for measuring the chemical force o rays, by the union of chlorine and hydrogen.

the first case of a movement, perceptible to the eye, being traced to the unaided action of light. That this unponderable produced molecular changes, was readily admitted; but its influence, in bringing about palpable movements of considerable extent, has never been suspected. In the irritability of the *iris*, physiologists have always seen the influence of nervous matter; but in plants no such agent exists to complicate the phenomenon; and, therefore, the action is due to light only.

In this newly discovered property light is also more closely assimilated to the other unponderables; for both heat and electricity are capable of producing palpable motion.

3. *Some applications of the preceding facts, &c.*

(35.) Numerous applications to vegetable physiology will suggest themselves to the reader; but it is my purpose to treat only of the following:

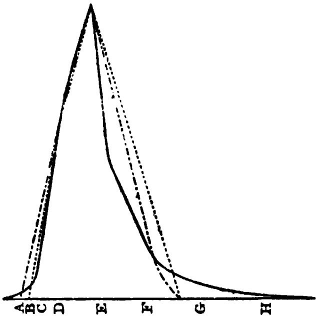
The intimate relation which exists between the rays which produce chlorophyl, the decomposition of carbonic acid, and the luminous spectrum. The maximum for the formation of green matter has been shewn to reside in the yellow ray. Dr Draper (Lond. and Edin. Phil. Mag., Sept. 1843) discovered the maximum action, for the decomposition of carbonic acid, to be between the green and yellow; or, more correctly, in the centre of the yellow. Sir W. Herschel and Fraunhofer placed the maximum for light in the same space.

(36.) The relation goes further; for if the quantities obtained by Dr Draper for decomposing action, as measured by liberated gas—Fraunhofer, for illuminating power, determined by the eye—and my estimate, obtained in time, and by the eye—be rendered commensurable and tabulated, they will give quantities nearly allied. To produce such a table, I assume all the maxima equal to unity. My results being in *time*, and theirs in *effect*, the inverse proportion is taken for each value given in Art. 14.

TABLE, *shewing the Force of the Solar Rays in producing the Green Colour of Plants, the Decomposition of Carbonic Acid, and Illumination.*

Places Examined.	Production of Chlorophyl.	Decomposition of Carbonic Acid.	Illuminating Power.
Extreme red	0·000	0·0000	0·0000
Line B	...	·0691	·0820
Line C	...	...	·0940
Commencement of orange	...	·5550	...
Line D	...	...	·6400
Centre of orange	·777	...	...
Centre of yellow	1·000	1·0000	1·0000
Line E	...	...	·4800
Centre of Green	·583	...	...
Line F	...	...	·1700
Centre of blue	·100	...	...
End of blue	...	·0027	...
Line G	0·000	0·0000	·0310
Line H	0·000	0·0000	·0056

Upon projecting these numbers, which, although not rigorously correct, are very good approximations, the unity of the active agent will be more strikingly exhibited. Let the axis of abscissus be divided into intervals corresponding to Fraunhofer's coloured spaces, and the positions of the mean places of the dark lines be marked from Powell's recent work on Dispersion. The ordinates are from the table. Fraunhofer's estimates are indicated by a bold line, Dr Draper's by dots, and my own by an interrupted line, fig. 1.



Had more points in these figures been determined, there is no doubt they would have coincided precisely. It is not to be forgotten, that these results were obtained in places many hundred miles apart. They determine, what hitherto has only been conjectured, that the greening of plants, and decomposition of carbonic acid, are produced by the same agent—which is also the active unponderable in producing vision—a phenomenon in no way similar, as suggested by M. Moser, to the change of Daguerre's plate, which is a tithonic action. The dependence of the depth of green colour in foliage upon brilliant light, is also shewn. The statements of travellers, in respect to tropical vegetation, confirm this conclusion.

(38.) Chlorophyl, the body generated in the yellow leaflets of plants, raised in darkness by the action of light, is a hydrocarbon, of the nature of wax. Whether it be produced by decomposition of carbonic acid, or be the yellow matter, or some other substance, as dextrine, already present in the leaf, which has suffered deoxidation, is altogether unknown. The latter view, applied to the formation of oils and fats in animals, by Liebig, is probably correct; by adopting it, we are relieved from all difficulty in regard to the supply of hydrogen in plants; for the evidence that water is decomposed in their structures, is by no means conclusive. In the formation of oils in seeds, it is known that the deoxidation of sugar occurs; for we have the liberation of carbonic acid from the petals, &c., and a destruction of the organic matter.

Subsequently to the production of chlorophyl, carbonic acid is decomposed by light, and this function, directly or indirectly, is sufficient to generate all organic matter. Hence the existence of all organic matter is due to the light of the sun.

(39.) *On the destruction of Chlorophyl by Light.*—The production of green matter by the yellow rays, leads us to infer its destruction by the blue and red. Sir J. F. W. Herschel (Phil. Mag., Feb. 1843) found that the expressed juices of leaves are acted upon by the spectrum with much uniformity. In the case of elder leaves (fig. 8), there was a strong maximum, producing a nearly insulated solar image at — 11.5 of his scale,* or nearly at the end of the red rays,—the action thence was feeble, with two minima at — 5.0 and + 6.8; with a slight intermediate maximum at (0.0) the centre of the yellow, and beyond these, or about the termination of the green, the action again increases, reaches another maximum at + 20.0, which corresponds to the centre of Fraunhofer's indigo, after which it declines to a point beyond the violet + 45.0.

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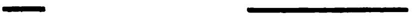
* By proceeding as in art. 13, a spectrum is obtained which has only the width of the focal picture of the sun, and is of considerable length; these elements differ, however, with the focal distance of the lens. Upon examining such a spectrum through cobalt-glass, a perfectly circular image of the sun is seen at the extreme red end, another in the centre

I have been thus precise in giving his result, because my experiments made with ethereal solution of chlorophyl from grass leaves spread upon paper, gave similar spectra. There are two points, however, which it is necessary to discuss.

The first action of light is perceived in the mean red ray, and it attains a maximum incomparably greater at that point than elsewhere; the next place affected is in the indigo, and, accompanying it, there is an action from + 10.5 to + 36.0 of the same scale, beginning abruptly in Fraunhofer's blue. So striking is this whole result, that some of the earlier spectra obtained by me, contained a perfectly neutral space from — 5.0 to + 10.5, in which the chlorophyl was in no way changed, whilst the solar picture in the red was sharp and of a dazzling whiteness, and the maximum of the indigo was also bleached, producing a linear spectrum, as in fig. 2; in which the orange, yellow, and green rays are inactive; these, it will be remembered, are energetic in forming the green matter.

Fig. 2.



Red, orange, yellow, green, blue, indigo, violet.

Upon longer exposure, the subordinate action along the yellow, &c., occurs, but not until the other portions are perfectly bleached.

In Sir J. Herschel's experiments, there remained a salmon-colour after the discharge of the green. This is not seen when

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of the yellow, and the termination of the violet is sharp and distinct. Sir John Herschel takes the centre of the yellow, thus insulated, as his zero point, and, using a scale of thirtieths of an inch, divides the distance between it and the red end into negative parts, and in the direction of the violet positively. The spectrum he used contained 13.30 negative, and 40.62 positive parts, and was, therefore, $\frac{53.92}{30}$ inches long. My spectrum corresponded with this very closely.

chlorophyl is used, and is due to a colouring matter, insoluble in ether.

(40.) No ground exists, therefore, for the theory that the autumnal tint of leaves is due to the residual, after the destruction of the green colour. Xanthophyl, which imparts the yellow, depends on an organic change of chlorophyl, which Berzelius could not imitate (Journ. de Pharm., Julliet, 1837).

Some observations made with a view of determining the action of indigo light on the green of living plants, brought me to the conclusion, that it faded into a yellowish-green colour; but I will not speak positively. Plants do, however, lose all their greenness in a dark place, after a greater or less time, and become the colour of seedlings raised without light. In this result my experience is at variance with the statement of Macaire Princep, "Les feuilles d'une plante conservées à l'abri de la lumière s'en detachent colorées vert." (In Berzelius, Chimie, t. 6, p. 42; from Mem. de la Soc. Hist. Nat. de Geneve, t. 4.)

(41.) *In the Bleaching of Chlorophyl*, as well as in its production, *the active agent* is light, for it will take place behind a medium excluding the tithonic rays; and the points of activity have no relation to the maxima of the calorific spectrum. See Sir John Herschel's paper (Phil. Trans. 1840, part i., p. 51, "On the distribution of the calorific rays of the solar spectrum".)

(42.) The coincidence shewn to exist between the illuminating power, activity of decomposition, and greening effect of yellow light, is conclusive of the discussion respecting the rays which are favourable to the growth of vegetables.

Blue light cannot be the best, as originally affirmed by Senebier, and subsequently maintained by Mr Hunt; nor would a conservatory glazed with cobalt-glass answer the expectations of Professor Johnston.

(43.) It is impossible to conclude without calling the attention of physiologists to the remarkable fact, proved in the second part of this paper,—that *indigo light* possesses a soliciting power, capable of governing the direction of the stems, peduncles, &c. of plants; an action accomplished by light incomparably feeble in comparison with the yellow rays. The blue of the atmosphere is scarcely less intense, when compared with the sun's beams. *Does not the colour of the sky, therefore, regulate the upright growth of stems to a certain extent? Is it not in virtue of the soliciting force therein, that plants continue to grow erect, whenever other disturbing forces are in equilibrio?* These questions might be investigated with profit, were not this communication too extended already.

(44.) It is proper to state, however, that De Candolle's theory of the bending of plants towards light, has been fully disapproved in the context,* inasmuch as it is effected by the indigo rays which have not power to decompose carbonic acid and produce lignin, &c. (See Mem. Soc. d'Arcueil, 1809, p. 104.)

In conclusion, it appears that the following facts have been established:—

1*st*, That chlorophyl is produced by the more luminous rays, the maximum being in the yellow.

2*d*, This formation is due to pure light, an imponderable distinct from all others.

3*d*, That the ray towards which plants bend occupies the indigo space of Fraunhofer.

4*th*, This movement is due to pure light, as distinguished from *heat* and *tithonicity*.

5*th*, *That pure light is capable of producing changes which result in the development of palpable motion.*

6*th*, The bleaching of chlorophyl is most active in those parts of the spectrum which possess no influence in its production, and are complimentary to the yellow rays.

7*th*, This action is also due to pure light. We have, therefore, an analysis of the action of every ray in the luminous spectrum upon vegetation. The several effects produced are not abruptly terminated within the limits of any of the spaces, but overlap to a certain extent, a fact which coincides with our experience of the properties of the rays. Whilst *heat* and

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* De Candolle advanced a theory to account for the bending of plants towards light on the following grounds. That as the side of any plant nearest the light was acted on thereby, whilst the distant portions were unilluminated, carbonic acid would be decomposed, and lignin, &c., produced on one side and not the other. The plant becoming firmer on the part thus furnished with woody fibre, bent over towards the luminous source.

tithonicity are capable of causing the union of mineral particles, *light* appears to be the only radiant body which rules preeminent in the organic world. To the animating beams of the sun, we owe whatever products are necessary to our very existence. (The American Journal of Science and Arts, vol. xlvi., No. I, January 1844, p. 1.)

Memoir of the late Mr William Blackie, Optician. By John Coldstream, M.D., Leith. Communicated by the Royal Scottish Society of Arts.*

William Blackie was born at Bainfield, near Edinburgh, 24th May 1808. He was brought up by his maternal grandfather, Mr George Blackie, gardener, whose name he assumed in after life, in preference to that of his father, in consequence of the latter having basely deserted his mother.

After having received the elements of education in various suburban schools, he began, at the age of twelve years, to work in his grandfather's garden at Upper Hermitage, South Leith. Nothing remarkable appeared in his character or dispositions during his boyhood; but, when he had reached the age of seventeen years, he became impatient of the monotony of a gardener's life; and, along with a cousin, suddenly left his home and went to sea. He was received on board of a coal-brig trading between North Shields and London, in which he met with treatment so harsh as to lead him speedily to repent of the rash step he had taken. While at London, he sought long and anxiously for employment in some of the large gardens near the metropolis, but in vain; and, at last, he was compelled by hunger to return to the vessel. Having completed the homeward-voyage, he felt that he had had enough of a sailor's life, and penitently returned to his friends within two months after absconding. He then resumed work in the garden.

It was about two years after this that an incident occurred which gave a decided turn to his occupations, and influenced the whole course of his future life. While digging in the garden, he happened to turn up the bottom of a chrystal tumbler; it was concave; and, on looking through it, he observed that it diminished the object. He then thought whether, if the glass were convex, it would have the effect of magnifying the object. This he resolved to bring to the test of experiment; and immediately proceeded to grind the piece of glass into a convex form. At this time, he was entirely ignorant of optical science; nevertheless, after long and hard labour, by means of a common grindstone, he succeeded in giving his glass the desired form; and he polished it with earth. He then found that, rough as his workmanship was, the convex glass realized all his expectations.

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* Read before the Royal Society of Arts on 8th April 1844.

Delighted with his success, he now resolved to attempt the construction of a telescope. With this view, he procured two pieces of glass, which he formed into lenses in the same manner as the bottom of the tumbler. He could, at first, procure no better tube in which to fix his lenses than a cabbage-stalk, hollowed out. One day, while he was enjoying the performance of this rude instrument near Lochend, he was accosted by a young man of the name of Forbes, who held in his hand a properly constructed telescope, and who proposed to Blackie to exchange instruments for a few minutes. Blackie shewed much reluctance to allow a stranger to inspect his telescope, but at last yielded to the entreaties of Mr F., who was much delighted and astonished at the effect produced by means so rude and simple. This interview led to the formation of a friendship which continued throughout life. It was at Mr Forbes's suggestion that William began to attend the lectures delivered at the Leith Mechanics' Institution, on Mathematics, Chemistry, and Mechanical Philosophy, which he did in the Session 1827–28. He continued to attend these lectures diligently until 1832, acknowledging that he derived from them much benefit. He also actively availed himself of the use of the excellent library connected with the same institution, devoting his attention more particularly to works on practical mechanics and optics.

These studies occupied only his evening hours until 1832, when, for a time, he abandoned the spade, and confined his labours, almost exclusively, to practical optics, in hopes of being able to obtain a livelihood by disposing of lenses and other apparatus, in the construction of which he now began to evince considerable dexterity.

He persevered in this for about twelve months, when, finding that his success did not equal his expectations, he once more returned to his former employment. For a short time he laboured in the Experimental Garden at Inverleith, but left it abruptly, in consequence of what he considered bad usage.

He now thought of becoming an engineer; and, with this view, endeavoured to procure employment in some engine factory, but in vain. This fresh disappointment brought him back to his lenses with new zest; and he spent the four remaining years of his life almost entirely in the manufacture of these, and that with a degree of success, as to the excellence of their performance, that has rarely, if ever, been equalled. The fact of Blackie's attaining such eminence as an artificer in work so delicate and minute as that of the construction of microscope-lenses of high power, notwithstanding his having been fully exercised for many years in the use of so large a tool as the spade, may be regarded as a very interesting addition to the history of the human hand; and as supplying another striking proof of the wonderful plasticity with which it has been endowed by the all-wise Author of our being.

It was about this time (the beginning of 1834), that he succeeded in forming a diamond lens having a focus of 1-90th of an inch, and a radius of 1-35th of an inch, which magnified 549 times. This, it is believed, was the first lens of the kind ever made in Scotland. His successful effort was the means of bringing him and his works under the notice of Sir David Brewster, Professor Forbes, and other philosophers, who subsequently employed him in various pieces of work connected with practical optics, and continued sedulously to interest themselves in his welfare to the close of his life. Encouraged by such patronage, he continued his work steadily, but still in the most quiet and simple manner possible. He usually wrought at a common table in his grandfather's kitchen; all his moulds and tools were made by his own hands, and were so insignificant in appearance, that they were seldom recognised even by persons conversant with the like pursuit. Some of his processes in lens-grinding and polishing were entirely of his invention; and, indeed, in almost every step of his progress, he strove to master difficulties by the exertion of his own ingenuity, rather than have recourse to the advice or assistance of others. To Mr George Saunderson, however, he acknowledged himself indebted for his having described to him a simple method of executing the spherical surfaces of garnet lenses.

By unremitting exertions, he produced numerous microscopic lenses of excellent workmanship and high power in the course of four years. He was particularly successful in making the bird's-eye lenses, or grooved spheres, now so much used as pocket microscopes. He also brought to great perfection achromatic object-lenses for the compound microscope. These have been pronounced by competent judges to be entirely free from spherical aberration, and to afford an extended field of vision. The following testimony to the surpassing merits of one of these lenses is extracted from the article "Microscope," in the last edition of the Encyclopædia Britannica:—"Mr Pritchard remarks, that when the lens next the object is a jewel, the performance of the doublet is improved; but that he has not observed any advantage when both lenses are gems. This must be a mistake; for lenses of any gem that are superior to glass ones when acting singly, must, if suitably combined, be superior also when united. In proof of this we have a garnet doublet before us, executed by Mr Blackie, the performance of which is quite remarkable. The lenses are made of Elie garnets, and their convex sides are placed towards each other. The radius of the smallest lens near the object is 1-70th of an inch, and that of the other 1-20th of an inch. Its magnifying power is very high, exceeding greatly that of the semi-jewel doublet made by Mr Pritchard, with a sapphire lens 1-60th of an inch focus, combined with a glass lens 1-10th of an inch focus."

After having so far distinguished himself by these beautiful works, Blackie was encouraged to visit London in 1836, partly with the view of making himself acquainted with the state of the art to which he had devoted himself, as carried on in the metropolis, and partly with the hope of obtaining some orders for pieces of work. He carried with him letters of introduction from Sir David Brewster to Mr Robert Brown, Mr Pritchard, Mr Bate, and Mr Jackson Lister. Mr Pritchard received him with great kindness, and gave him orders for some of the grooved spheres. He was favoured also with the patronage of several other distinguished philosophers and fellow-artists; and he returned home both pleased and refreshed by the agreeable incidents of his journey, and by the excellent opportunities of improvement which he had enjoyed.

Mr Blackie now made for the late Mr Sivright of Meggetland, a diamond lens similar to that which he manufactured in 1834, which, it is believed, remains in the possession of Mr Sivright's family. He also made, about this time, a number of lenses of various kinds for Professor Forbes; and assisted that gentleman greatly, by grinding plates and lenses of rock-salt, which were used in the experiments on light and heat, subsequently communicated to the Royal Society by the Professor. It is understood that no other artist was found able and willing to undertake this labour.

Mr Blackie also made a fine sapphire-lens, focus one-seventieth of an inch, radius one-fortieth of an inch, which magnifies 420 times; a garnet-lens, one-fiftieth of an inch focus, magnifying 300 times; and a smaller garnet-lens, one-twentieth of an inch focus, magnifying 720 times.

But the subject of our memoir was not a mere artist. He carefully and successfully cultivated the higher powers of his mind, and did much to supply the deficiencies of his early education. He was fond of plants, although he disliked the labours of the gardener; and he sedulously cherished a fine collection of choice flowers, of the produce of which he made his friends partakers. He made good progress in the acquisition of botanical, chemical, and astronomical science. He was, above all, eminently characterised by extreme modesty, and perfect simplicity and ingenuousness of disposition. His deportment was particularly unassuming. He was most kind, attentive, and obliging, in all the relations of life; and he was most warmly loved by all who knew him best.

It is probable that intense application to his labours laid the foundation of the disease—consumption of the lungs—of which he died. The symptoms of the fatal malady began to manifest themselves in the autumn of 1836; but he was able to continue at work, although with many interruptions, until within two or three months of his death, which happened on the 15th January 1838.

Throughout the whole of his illness, he maintained the same meek and quiet deportment which ever distinguished him. He spoke little; but that little proved how much his spirit was cheered by Christian faith and hope, and discovered on what a deep foundation were based the remarkable humility, simplicity, and sincerity, which formed the chief ornaments of his character, and endeared him to all connected with him.

March 30. 1844.

Description of Portable Levelling Instruments. By DAVID STEVENSON, F.R.S.E., F.R.S.S.A., Civil Engineer, Edinburgh. Communicated by the Royal Scottish Society of Arts.* With a Plate.

IN examining a tract of country, I have often experienced the want of some portable, and, at the same time, accurate instrument, for ascertaining, in a general manner, the relative levels of different points, previous to determining the line of a more detailed survey. A small spirit-level without any telescope, having a common sight and cross-hair attached, is sometimes used for that purpose, being fixed on a staff stuck into the ground. The large instruments commonly used in levelling are also often employed, but neither of these instruments answers the object I had in view; the first being much too rude for the required accuracy, and the second too heavy for easy transport. The level represented in the accompanying drawing (Plate 1st), was designed to supply this want. It consists of an accurate spirit-level, a 10-inch telescope, and a compass, so arranged as to admit of being very portable. The telescope unscrews at letter A, so as to form two compartments, and the whole is packed in a pocket case measuring 6 by 2¾ inches; and the tripod on which it stands does not exceed the bulk of a thick walking staff.

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* Read before the Society, 12th February 1844.

Referring to the drawing, B C is the level, D a circular level, E the compass, F screw for adjusting focus, G the eye piece, H screw acting on spring K L, which is fixed to the telescope at M by a crutch on which it moves, N the screw by which it is fixed to the tripod, O P Q R the top of the tripod, which contains a ball and socket-joint, shewn in dotted lines, which can be clamped and unclamped by means of a screw wrought on the inside of the part S T. In setting the instrument, the screw S T is first unclamped, and the instrument is moved by the hand on the ball and socket-joint until the air-bubble of the level D occupies the centre of the circular box containing it. The screw S T is then clamped, and the instrument being directed to the object to be observed, the final and more perfect adjustment is made by bringing the air-bubble B C to occupy the centre of its tube, which is done by means of the screw H, which acts on the spring K L. The tripod is that used by Dollond for the camera lucida, and answers both instruments. The telescope can be made either, as in ordinary levels, to reverse the objects, or, as in theodolites, to shew them in their true positions. In this level made for myself, I have adopted the latter construction, in order that the instrument may answer more perfectly the purposes of a field telescope. The addition of the compass is also a further convenience.

In connection with this instrument, I have also had a portable levelling staff made, which also is shewn in Plate I. It consists of an elliptically-moulded staff, 3 ft. 3 in. in length, and cut through the middle; the two halves are hinged at one extremity, and when unfolded, are fixed by a spring at A, forming a rod 6 ft. 6 in. long, on the flat side of which the graduations of feet and inches are painted; when closed, the gradua-

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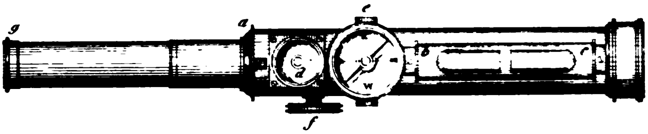
Small portable Levelling Instrument & Rod, constructed by Mess.rs Adie, for David Stevenson, C.E.



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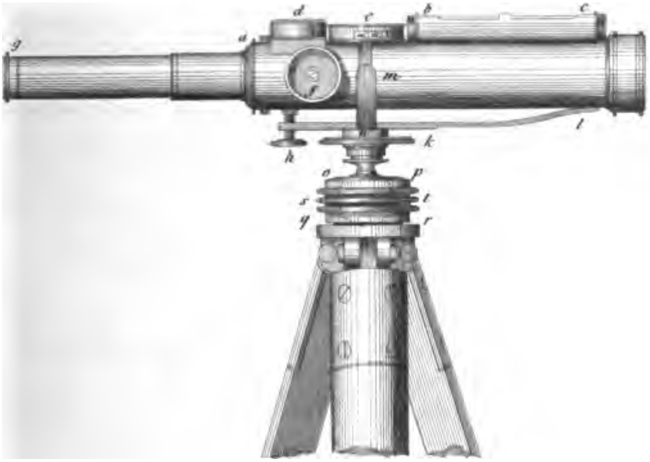

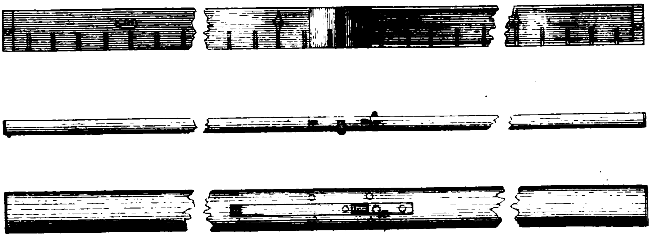

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tion is protected from injury, and the whole forms a convenient walking staff.

These instruments, which were made for me by Messrs Adie of Edinburgh, I have found very convenient; and, I am persuaded, they will be suitable in all circumstances where the combination of *accuracy* and *portability* is required.

David Stevenson.

Edinburgh, 11*th January* 1844.

Some Account of Levelling Instruments, with Description of one of an Improved Form. By Thomas Stevenson, Civil Engineer, Edinburgh. Communicated by the Royal Scottish Society of Arts.*

The improved spirit-level about to be described, was made for me in 1840 by Mr Adie, and afterwards described in a letter to the Institution of Civil Engineers.†

Since then I have had two levels of the common construction altered to the new form. On a late occasion these instruments were exhibited to the Royal Scottish Society of Arts; and as no drawings had been laid before the public, that Society requested me to prepare a description with diagrams, that these might be published in their Transactions. In accordance with their request, therefore, I have drawn up the present description; and have also consulted many early writers, to ascertain, as well as I could, the history of one of the most important of geodetical and engineering instruments.

There unfortunately exists a great deal of conflicting evidence regarding the true inventors of the different parts of the spirit-level. Indeed, there are hardly two authorities who agree upon the subject.

The great Huygens appears to have been the first to *apply the telescope to a level* of his, which was constructed on the principle of the plummet. This is noticed, in the life prefixed to his works,* in the following terms:—"Ibi" (Lutetiæ) "vixit ab anno 1666 ad annum 1681. Durante hoc tempore pulcherrima subtilissimaque multa in mathematicis detexit variaque ex iis operibus conscripsit quæ nunc in unum corpus collecta quid in variis Matheseos partibus præstiterit sub oculis ponunt. Præter ipsius jam memorata inventa præclara inter alia duo insigni usu eminent. *Libellam telescopio munitam* ita construxit ut ipsi præ ceteris fides haberi possit," &c.

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* Exhibited and verbally described to the Society on 22d January 1844; and this written description requested.

† Engineer and Architect's Journal, vol. v.

The honour of having first applied the *air-bubble* to the determination of horizontality seems to be due to that universal genius Dr Hooke. From all that I can gather, it appears that his invention must have been made subsequent to 25th March 1674, and prior to the year 1675, as, in his "Attempt to prove the Motion of the Earth by Observations," of date 25th March 1674, he describes a new method of *stilling the plummet* by immersion in water. While in his animadversions,† published also in 1674, after fully describing his invention of the air-bubble confined in a tube, he speaks of its peculiar advantages, and great delicacy of movement, and remarks,—"This can hardly be performed by the ordinary way of plummets, without hanging from a vast height, which is not practically to be performed without almost infinite trouble, expense, and difficulty," &c.

Hutton, in his Mathematical Dictionary, remarks, that the application of the air-bubble to the level "is said to be due to M. Thevenot;" but with what justice I cannot say, having been unable to meet with any reference to this instrument in the writings of that author. Thevenot was born in 1621, and he died in 1692.

I have been unable to discover who was the inventor of the *circular level*, which I imagined had been of recent date; but Switzer, at page 91 of his Treatise on Water-works, which was published in 1734, remarks, that the circular level was then employed in the construction of the surveying instrument called a Plane-table.

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* Christ. Hugenii Op. Var. Lugd. Batav. 1724.

† Animadversions on the first part of the Machina cælestis of the Hon., learned, and deservedly famous Astron. Johannes Hevelius, Consul of Dantzick, together with an explication of some instruments made by Rob. Hooke, Prof. Geom. in Gresh. Coll., and F.R.S. Lond. 1674, p. 61, *et seq.*

According to Sir John Herschel, *the cross-hair*, which gives so much accuracy to all astronomical as well as levelling instruments, was the invention of Gascoigne, a young Englishman, who used it in 1640. He was killed at the age of 23, at the battle of Marston Moor.

M. Le Bion[*] appears to have been the first to *conjoin the telescope of Huygens with the air-bubble* of Dr Hooke; and this must have been subsequent to the year 1684, as such an instrument is not shewn in De La Hire's edition of Picard's Treatise on Levelling.[†]

But it was not till Sisson's improvements that the level could be considered as in any way an accurate or philosophic instrument. All that were made previous to his time were coarse instruments, adjusted by a ball and socket, and in other respects resembling the common perambulatory survey-level, which, from the nature of the construction, can be levelled in only one direction, and cannot be reversed, or moved even in the slightest degree, without requiring readjustment. Sisson may, therefore, be considered as the inventor of the instrument in common use. The main feature in his improvements was the introduction of the four screws called *the parallel plate-screws* (D, in the Diagram). I have been unable to find out the date of Sisson's improvement; and, indeed, the only notice I can find of him is the following in Switzer's System of Water-works: "The invention" (alluding to the instrument with parallel plate-screws) "as I take it (for I am not as yet well acquainted with that gentleman), of William Sisson, at the corner of Beaufort Buildings, in the Strand."[‡]

Since the time of Sisson, the celebrated Ramsden introduced a tangent-screw and clamp, for moving the instrument with accuracy through small distances in an arimuthal direction. Messrs Troughton and Simms also made several improvements in the arrangement of the various parts of the instrument; and Mr Gravatt has of late years added a cross-bubble for facilitating the *rough-setting* of the instrument—or that adjustment which is made with the legs of the tripod; and an enlargement of the diameter of the object-glass, so as, by the admission of a greater number of rays of light, to allow of the telescope being shortened, without impairing its optical powers.

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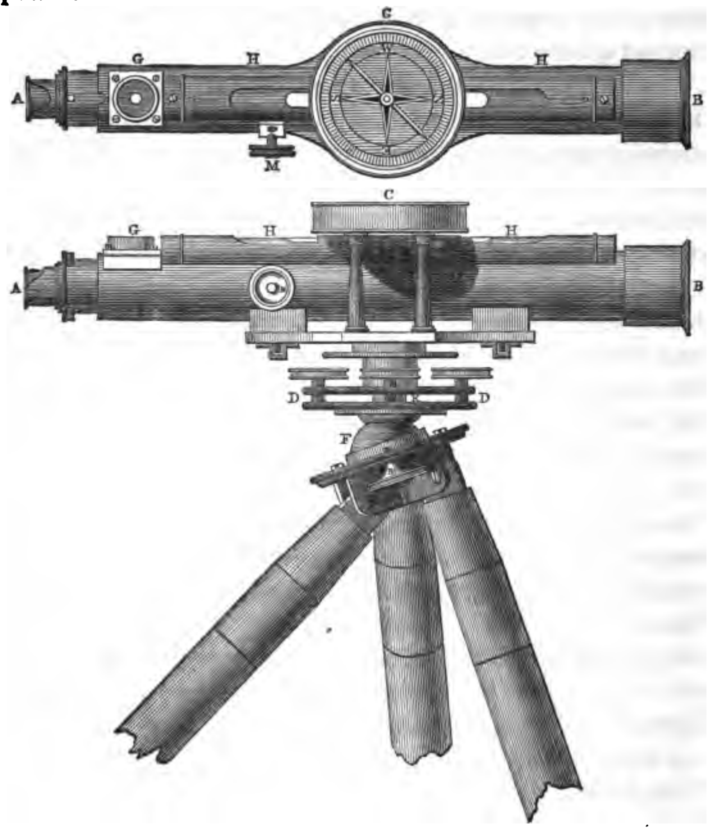
[*] Traité de la Construction et des Principaux usages des Instrumens de Mathematique. Par N. Le Bion, Ingénieur du Roi pour les Instrumens de Math. Nouv. Edit. A La Haye, 1723.

[†] Traité du Nivellement Par M. Picard, mis en lumière par les soins de M. De La Hire, 12mo. A Paris, 1684.

[‡] An Universal System of Water and Waterworks. By Stephen Switzer, 2 vols. 4to. Lond. 1734.



A B is the telescope.
C C the compass-box.
M the screw for adjusting the focus.
H H the tubular spirit-level.
G the spherical or circular level.
D D D the parallel plate-screws of Sisson.
K the old ball and socket motion.
F the new ball and socket motion.
N clamping-screw for ditto.

Having thus endeavoured to describe the successive changes which the level has undergone, I shall now proceed to notice the nature of the present improvements.

The first of these is the substitution of a circular, or, to speak more correctly, a *spherical* level (G), sluggish in its motions, instead of the small cross-level, which was introduced by Mr Gravatt. The advantage of the circular level over the common form, is its peculiarity in at once shewing the deviation of the instrument from horizontality in both directions, instead of only one.

Before describing the next improvement, it may be proper to state, that the clumsiness of the common level consists in its being at all dependent on the setting of the legs. This arises from the circumstance of the ball-and-socket motion (K) being controlled in its action by the parallel plate-screws of Sisson (D), the consequence of which is, that, in using the common level, care must be taken to set the instrument very nearly level *by the eye*, so as to be within the range of the parallel plate-screws (D), otherwise it is impossible to adjust the instrument. And although to the practical man, the trouble attending this may be comparatively small, still he will admit that it is one of the most irksome parts of the whole operation of levelling; to say nothing of the time that is lost in adjusting the instrument afterwards with the parallel plate-screws. What appeared to be wanting was a motion for the preliminary, or *rough-setting*, intermediate in nicety between those of the parallel plate-screws and of the legs. In order to gain this end, a ball-and-socket motion (F), having a clamp (N), is introduced in addition to the ball-and-socket (K), whose action is limited by Sisson's parallel plate-screws (D); *so that my improved level has two ball-and-socket movements.*

With the instrument thus improved, the observer is made quite independent of the level of the ground where he sets the legs of his instrument, and may place them without regard to the inclination of the telescope to the horizon. Looking first to the circular level (G), and releasing the clamp (N) of the ball and socket (F), he, with one hand, moves the head of the instrument till the bubble is in the centre of the circle, an operation which is done almost instantaneously.* The socket-screw (N) is then clamped, and the telescope bubble (HH) is brought to the *absolute* level by a slight touch of the parallel plate-screws (D). In this way the legs of the tripod never need to be moved after the instrument has been placed on the ground, and the parallel plate-screws have almost nothing to do—advantages which all who are accustomed to levelling will fully appreciate.

In levelling over mountainous districts, it very often happens that it is desirable to select a station where the ground is so rugged and precipitous as to render it difficult, if not impossible, to find three points for the extremities of the legs of the instrument to rest on, which shall be on such levels as to bring the telescope within the range of the parallel plate-screws; *but wherever the instrument can be made to stand with safety, the bubble of the improved level can be adjusted, and adjusted in exactly the same time, and with exactly the same ease, as if the instrument were placed on level ground.*

Another advantage of these improvements is the removal of a great practical difficulty which is often experienced on sloping ground. The instrument being set and properly adjusted, the observer, on looking through the telescope, may discover that he is not within the range of the levelling-staff; in other words, he has chosen a station too high or too low to admit of his seeing any part of the staff within the field of the object-glass. The only remedy for this is to choose a new station where the instrument must be again set up and levelled, at a great expense of time and trouble. In order to remedy this, it was my intention at one time to have fixed on the telescope a French level, on the principle of the plummet, in order speedily to discover, before making the adjustments, whether the intended station were within the range of the staff or not. But the instrument can be roughly set with so much quick-ness by means of the additional ball and socket, that the French plummet may be considered as being now scarcely necessary.

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* In the annexed plan the instrument is shewn off the level, so that neither the air-bubble of the circular level (G) is in the centre of the circle; nor does the air-bubble in the tube (H H) correspond with the file-marks made on the glass.

In my letter to the Secretary of the Institution of Civil Engineers, I pointed out the advantages which would result to the surveyor were the theodolite provided with a second ball and socket motion; but no opportunity of trying this has as yet occurred.

EDINBURGH, 1844.

An Inquiry into the Nature of the Simple Bodies of Chemistry. By DAVID LOW, F.R.S.E., Professor of Agriculture in the University of Edinburgh.

In a former Number we referred to this work, and we do so again, in order that we may enter our early protest against certain attempts that have been recently made, not to refute the arguments employed, but to run down the author. Fortunately for the interests of truth and science, offences of this kind have now become rare. Mr Low's argument will be best stated in his own words:

> "The greater number of substances with which we are conversant, are derivable one from another, and are therefore termed Compound; but of the numerous class which we term simple, many are similar to one another with respect to their essential characters, and pass the one into the other by scarcely perceptible gradations, nay, pass into those we term compound, so that no line of natural division can be drawn between the two classes. Yet we hold the one class to be derivative or compound, and the other to be derived from no other bodies; but to be, as it were, distinct products of nature, each formed of particles proper to itself. It is not enough that we explain the meaning which we attach to the term simple, as applied to these bodies, by saying that we hold them to be simple, because we are unable, by the means at our command, to resolve them into other bodies more simple. This is the mere expression of a fact; but even were the fact established beyond dispute, which it is not, we should not be entitled to regard the bodies in question as simple, in contradistinction to another class which we regarded as compound. By the terms simple and compound, we indicate two Orders of bodies, the most distinct, with respect to their chemical constitution, which we can conceive to exist in nature. But there is no such distinction in the chemical and physical characters of the bodies themselves, as can warrant us in assuming that they are distinct in their nature. The mere circumstance of our inability to compose or decompose the substances in the laboratory, furnishes, at the best, merely negative evidence. Superior means of analysis, or a better use of the means we possess, may

enable us to prove bodies to be compound which we now hold to be simple. But even were it otherwise, we have other means of investigation than the processes of the laboratory, for conducting us to truths in science. We have induction and analogy, without which even experiment would fail to conduct us to the discovery of natural laws. If the bodies which we term simple, present the same general physical properties, and exert the same chemical actions, as those which we term compound, and pass into the compound bodies in their characters and functions, the merely negative evidence, that we are unable to decompose them by overcoming their chemical affinities, should not invalidate the conclusion, that both classes are to be placed in the same order of natural bodies, and cannot be separated the one from the other, by so wide a chasm as a distinct molecular constitution."

Having enunciated his proposition, the author proceeds to the inquiry, whether, consistently with the known laws of chemical combination, we can conceive the bodies regarded as simple to be resolvable into any other bodies more simple of their own order. He makes the supposition that they may be resolved into *three*, having the lowest atomic weight, namely, hydrogen, carbon, and oxygen. He thus adopts the method of reasoning so well known, of assuming certain premises, and determining their truth or error by the conclusions arrived at. This is done by a review of all the undecomposed bodies of chemistry, and of their relations with one another, and with the bodies which we know to be compound. The results are in many cases remarkable, but in no case, as far as we can perceive, inconsistent with the laws of chemical combination, as determined by experiment.

But of the three bodies referred to, one or more may be compound. Pursuing the same mode of reasoning, one of them is assumed to be compound, and this one to be oxygen; because, while the known atomic weights of hydrogen and carbon, 1 and 6 respectively, will allow us to suppose one or both to be resolvable into oxygen, the atomic weight of oxygen, 8, will not allow us to resolve oxygen into hydrogen or carbon. This is the second stage of the argument, although carried on coincidently with the first. The author, therefore, makes the supposition, that all the undecomposed bodies may be resolvable into *two* of their own order, namely, hydrogen and carbon; and this supposition is to be tested, like the other, by its accordance or disagreement with the results arrived at. It is remarkable, that this stage of the hypothesis, though arrived at by a different train of reasoning, agrees with a favourite speculation of Sir Humphrey Davy, who supposed that all the simple bodies of chemistry, so called, might be resolvable into two—hydrogen and an unknown base. Under the present supposition, the base is inferred to be a *known* body, namely, carbon, or the elements of carbon.

But hydrogen and carbon may, one or both, be compound bodies. The author, pursuing the train of his reasoning, shews, that we must then derive them from an order of molecules superior to that of known forms of matter. We are, therefore, directly led to a conclusion, favoured by the most eminent metaphysicians, regarding the nature of matter, namely, that all matter is derived from some common form of existence. The author arrives at this generalization; but he does not pursue it to its results, because, as he states, it is not necessary for his argument, which is designed simply to shew that the bodies termed simple cannot be separated, as a distinct order of natural products, from the bodies which are determined to be compound, by experiment.

It is difficult to comprehend why a speculation like this, within the fair range of philosophical inquiry, should have been received with so much bitterness by certain chemists. Are they afraid to have preconceived opinions shaken by the progress of inquiry? It must be confessed, that the author presses hard on some favoured opinions. He will have it, that chlorine is no more a simple body than cyanogen, with which it preserves the closest parallelism in its chemical actions; that ammonia is not distinguished from the other alkaline bodies by any essential difference in constitution; that if ammonium, H^3N, be a metal, all the other metals must be compound. He makes light of the many chemical formulæ held to be established by something little short of demonstration. He asserts that we know nothing of the mode of existence of a compound molecule, and that all that a chemical formula can express is, the number and ratio of the elements which enter into any given body, or which are derived from it. We express no opinion upon this, or the author's other views. We only say that he argues the question fairly and temperately; and we doubt not he is able and prepared to defend his opinions, and answer every fair objection.

Notice of the Employment of the Flesh of Small Whales for feeding Cattle in the Faroe Islands. In a letter to the Editor from W. C. Trevelyan, Esq.

I yesterday received a letter, dated June 2d, from the Faroe Islands, which contains further information regarding the capture of whales by means of nets, of which a notice appeared in the Journal for January. The total number of the Delphinus melas (Caaing whale) taken in Faroe in 1843, was 3146, besides a few individuals of other species; most of these were captured by means of the net before mentioned. The quantity of oil obtained from the blubber and exported, was 87,404 gallons, and its value L.5665; besides this, about one-eighth of the blubber was salted for food, and some oil reserved for domestic uses, &c. During the past winter, a novel but important experiment has been tried with the flesh of these animals:—it was then for the first time used as food for cows, and apparently with perfect success. For this purpose the flesh is cut into long and narrow strips, and dried, without salt, in the air, in the same manner as when used for food by the natives; when well dried it will keep good for two years. When used, it is cut into pieces two or three inches long, and slightly boiled; any oil rising to the surface is skimmed off, and then the soup and meat are given to the cows, together with about one-half or one-third the usual quantity of hay. On this food they appear to thrive well, giving an increased quantity of milk; and neither it nor the cream has any unpleasant flavour, as they have when the animals are fed on dried fish, as in Iceland and other northern countries. Many cows have usually perished in Faroe from the scarcity of fodder in winter; and my correspondent, the Rev. Mr Schroter, (who has for many years exerted himself in improving the condition of his fellow countrymen), calculates that the lives of more than 600 cows were saved last winter by the use of this food; which, he remarks, might be found of value for the same purpose in Shetland and Orkney, where, from the flesh of the Delphinus being disliked as food, great quantities of it are wasted which might be profitably employed in this way—a more valuable application of it than for manure, as formerly suggested; and if the supply were at all regular, it might enable the inhabitants to increase their stock of cows in winter, and thus add much to their domestic comfort.

EDINBURGH, 25*th June* 1844.

Report on M. Alcide D'Orbigny's Memoir, entitled General Considerations on the Geology of South America. By M. ELIE DE BEAUMONT.

Concluded from vol. xxxvi. p. 62.

Erratic blocks.—The deposit of erratic blocks, not less mysterious than that of the loam formations, also exists in South America; but there, as in Europe, it is placed at the side of the loam, and appears to be parallel to it. The Pampean loam is rarely mixed with pebbles, and it is only so in the mountains. Messrs D'Orbigny and Darwin agree in saying, that there is not a single rolled pebble to be met with on the surface of the Pampas.† It is different in Patagonia, where the Pampean loam does not exist, and where the Patagonian tertiary formation is everywhere exposed. The surface of this tertiary formation appears, according to M. D'Orbigny, to have been furrowed by great currents of salt water coming from the west. It is those currents which, according to him, have not only formed vast depressions and extensive valleys, but have also every where left, at the surface of the rocks, a thin mixture of round and small porphyritic pebbles, derived, doubtless, from the rocks by which the Cordillera is composed. These porphyritic pebbles, distributed over the surface of the tertiary formations of a large part of Patagonia, do not extend over the Pampean loam. Their transport must, therefore, have been contemporaneous with the deposit of loam, or anterior to it.

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* From l'Institut, No. 540, p. 154.

† Darwin's Geology of the Voyage of the Beagle; Introduction, p. iii.

It appears that these pebbles increase in size towards the south, and at last pass into erratic-blocks. These blocks, distributed in great abundance over the southern extremity of the American continent, as they are over the northern extremity both of the new world and of Europe, were not observed by M. D'Orbigny, but they have afforded Mr Darwin a number of curious observations. The most northern point at which they were noticed by this distinguished traveller, on the plains of the eastern portion of South America, was on the banks of the river Santa Cruz, in S. lat. 50°, 10′, a latitude corresponding to that in which the phenomenon of erratic blocks derived from the north becomes much less intense in the northern hemisphere. Erratic blocks are not met with in Patagonia near the coast; in ascending the river of Santa Cruz, they have not been remarked nearer the coast than 112 English miles, and that 74 English miles from the foot of the Andes at the nearest point; they consist of compact clay slate, a felspathic rock, a very quartzose chlorite slate, and basaltic lava. They are generally angular, and their dimensions are frequently gigantic.*

What are the relations of these erratic blocks to the Pampean loam? The question is here the same as in regard to Europe and North America, because the blocks and the loam succeed each other in the same order of succession as we advance from the pole to the equator, and the blocks cease where the loam commences.†

Recent Alluvial Deposits.—The Pampean loam, although very recent, is, nevertheless, not the newest of the deposits which are spread over the surface of South America. It is itself covered by deposits of two different kinds, but which M. D'Orbigny regards as contemporaneous. On the great Bolivian Plateau, and in the province of Moxos, there are thick alluvial deposits whose age has been indicated to M. D'Orbigny by human remains. They are, according to him, all posterior to the commencement of our epoch. In the Pampas, there are, moreover, over a large extent of surface, *medanos* (ancient downs of sand); and near the coast, at Bahia Blanca, at San Pedro, &c., beds of shells analogous to those which exist at the present day in a living state in the neighbouring seas.

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* Darwin, on the distribution of the erratic boulders, and on the contemporaneous unstratified deposits of South America. (*Transactions of the Geological Society*, 2d Series, vol. vi. p. 415.)

† *Vide* Report on the Memoir of M. de Castelnau, Comptes Rendus, t. xvi. p. 535.

M. D'Orbigny was for a long time in doubt as to the age of the alluvial matters which cover the Pampean formation at the eastern base of the Andes, but an observation made in the province of Moxos enabled him to determine it. He found on the Rio-Securi a bank 8 yards in height, composed, at its lower portions, of 2 yards of Pampean formation, and above, of 6 yards of alluvial deposit. At a little distance from the Pampean formation, in the lowest beds of the alluvium, he discovered a great number of fragments of earthen-ware, which proved the former residence of the ancient inhabitants; this discovery afforded certain evidence that these alluvia (if they are all contemporaneous), are posterior to the creation of man.

At the extremity of the bay of San-Blas, at a place named *Riacho-del-Ingles*, M. D'Orbigny found superimposed on the tertiary sandstone, an immense arenaceous bed, containing, along with crystals of gypsum, a great number of shells of Gasteropoda and Acephala identical with those which now live in the bay. This bed, situated about a mile and a quarter from the sea, was half a yard above the highest tides. The shells were in the position in which they had lived, and the acephala had their two valves united. The tides in these latitudes rise upwards of eight yards; these shells occur about half a yard above the highest; at present they live at a distance of two miles and a half from thence, below the lowest tides. We may hence conclude that the shells in this bed are elevated about ten yards above their present position.

The environs of Monte Video present hills of gneiss, at the base of which reposes a bed of marine shells, at the height of four or five yards above the La Plata; the species are indeed different from those which live in the brackish water of the bay of Monte Video itself, but identical with those of the maritime coasts, at a distance of 74 miles nearer the mouth of the river. M. D'Orbigny observed in the neighbourhood of San-Pedro, on the *tosca* plains, about thirty yards above the coasts of the Parana, small mounds, about two or three yards high, having an elongated form, and, generally speaking, the same direction as the course of the Parana. These mounds are composed of very fine sand, and are so filled with shells that they have received from the inhabitants the name of *conchillas.* These shells belong to the species *Azara labiata,* which no longer lives near San-Pedro, and is not met with, descending the river, sooner than at Riacho-de-las-Palmas, not far from Buenos Ayres; it abounds in the fresh and brackish water of the mouth of the Plata. These mounds, whose thickness and extent are so considerable that they have been used for the manufacture of hydraulic lime, cannot have been accumulated by man. If, on the one hand, the state of preservation of the shells proves that they belong to a deposit contemporaneous with the human epoch, the fact of their two valves being united, and their perfect preservation, forbid, on the other hand, the idea of their having been transported, and prove that they lived at no great distance, if not upon the very spot where they are now found. These deposits are evidently connected with the cause which has given rise to the formation of the *medanos*, or ancient downs, which are found distributed very far from the sea in the centre of the Pampas, towards the south.

To the west of the Cordillera, analogous mounds, containing the shells which occur on the present coasts, have been observed at Talcahuano, at Coquimbo, at Cobija, at Arica, and at Lima, over an extent of upwards of 1600 miles.

The recent shells observed by M. D'Orbigny in the raised beaches of the two shores of South America, have given rise to two very interesting remarks. The first is, that all these shells have their analogues in the neighbouring seas, and that, as a whole, they exhibit on the two sides of the Andes as great a difference as is presented by the existing faunas of these two seas. Whence it necessarily results, that at the epoch when they lived the two seas were already separated. The second observation made by M. D'Orbigny is, that the recent shells of the raised beaches of the two coasts of South America, are all in the natural position in which they lived, the acephala having their two valves united and placed vertically. This fact supports the idea of a sudden movement, and not a gradual elevation of the coast, as has been supposed by some authors. The study of the present coasts proves, that when the sea gradually abandons a shore, it leaves, everywhere on the uncovered portion, shells, which are exposed for a long period to the incessant movement of the waves, and which soon become more or less rounded, no one remaining in its original position. Nothing of this kind presenting itself in the elevated deposits visited by M. D'Orbigny, it seems to him evident that these shells had been suddenly and instantaneously raised from the bottom of the sea to the height which they now occupy. This leads him to conclude that a sudden movement has taken place on the surface of America, whose traces are preserved, on the one hand, in the terrestrial alluvia; and, on the other, in the elevation of the marine beds of the coasts of the two oceans.

The terrestrial alluvia and the marine beds which cover the Pampean tertiary formation, would thus be contemporaneous with the species which now live on the globe; while the Pampean formation itself, from its terrestrial fauna being very different from the fauna of the present day, would belong to a very different anterior epoch, characterised by large animals of a lost race. Thus, while on the one hand, the Pampean formation seems to refer to a great event which destroyed the megatherium and the mylodon, it would seem equally probable, that since the existence of the present fauna there have been general and transient causes which, at the same time that they elevated a portion of the shore, as well of the Atlantic as of the Pacific Ocean, containing organised bodies identical with those living at the present day, have also denuded the plateaux and mountains, and have transported to the Pampas and to the plains of Moxos those immense masses of alluvial matter which are there observed, and whose modern origin is indicated, as we have already mentioned, by the products of human industry discovered by M. D'Orbigny at the Rio-Securi.

It is, doubtless, sufficiently difficult to trace with certainty the line of demarcation between the ancient raised beaches, and those beaches which are from time to time elevated by earthquakes on the coasts of Chili, as well as between the alluvia of the present day and the vast alluvial deposits of the great plains of the interior of America. Nevertheless, the fine sand, sometimes containing shells, which covers the Pampas; the *medanos*, or ancient downs of the same plains; the sands which form elongated hills in the east of the province of Corientes; the gravels and sands of the great Bolivian plateau; the immense alluvia of the environs of Santa-Cruz-de-la-Sierra, of the plains of Moxos, and of the province of Chiquitos; all these deposits, more modern than the Pampean formations, cover them too generally, and too uniformly, not to induce us to suppose that they are traces of a general phenomenon. The same is the case with the deep denudations, so different from those produced by ordinary running waters, which have furnished their materials.

Ancient Beds of Torrents.—This is the natural place for noticing one of the most curious observations made by the author. M. D'Orbigny has pointed out at Cobija, at Arica, and over the whole coast of the Pacific Ocean, ancient beds of torrents, which, subsequently to the last movements of the surface of South America, furrowed the whole slopes of the Cordillera, from the summits to the shore. He is convinced that these ancient beds of torrents, occurring in a region where it has not rained in historical times, have not been derived from local rains, but must be attributed to masses of water which descended from the Cordilleras alone. At the present day an aqueous cloud is never seen on the mountains of the western side; a patch of snow is never visible on this slope of the Cordilleras. In order, then, to explain these torrents whose traces are observed over a great space, it is necessary to suppose that the Cordilleras for a time received rains or snow which they no longer receive; an aqueous phenomenon must thus have occurred on the mountains analogous to that whose traces are visible on all the great mountains of Europe. These facts are remarkable in themselves, and the approximations to which they may give rise seem to us worthy of all the attention which the author has bestowed on them. They will remain as landmarks, without doubt still too few in number, in the midst of the discussions which they will not fail to originate.

General Observations.—From all that we have said, it thus appears that the stratified formations of South America may be divided, according to M. D'Orbigny, into eight very distinct groups:—1. The old crystalline formations, in which gneiss predominates; 2. The Silurian and Devonian transition formations; 3. The Carboniferous formations; 4. The Triassic formation; 5. The Cretaceous formations; 6. The Guaranian and Patagonian tertiary formations; 7. The Pampean loam; 8. The modern deposits, which he also terms diluvial, from the nature of the cause which has produced them.

These different groups of beds have positions which are altogether dissimilar and discordant; and, according to M. D'Orbigny, these discordances result directly from the dislocations which have changed the surface of America, and have given rise to the chains of mountains which traverse it. In the same manner as has been attempted in Europe, and as M. Pissis has attempted in regard to Brazil,* M. D'Orbigny has endeavoured to connect the interruptions of continuity presented by the series of American formations, with the successive appearance of the chains of mountains which form the principal features of the relief of South America. His classification embraces two systems of mountains already pointed out by M. Pissis. As we have already said at the commencement of this report, a very old gneiss formation presents itself over a great extent of country on the eastern coast of South America. It occupies the eastern portion of Brazil, to the east of the Mantiquiera, from the 16th to the 27th degree of S. latitude, and there forms a series of small chains, whose general direction is, according to M. Pissis, from east 38° north, to west 38° south. This system, which M. D'Orbigny names the *Brazilian system*, would seem to be one of the most ancient which we can trace through the posterior modifications of the crust of the globe. M. Pissis regards it as anterior to the transition formations of Brazil, and perhaps it preceded the *soulèvement* of the most ancient system of mountains hitherto described in Europe. It is probable that it affects to great distances the fundamental rocks of America; for the general direction which we have just indicated differs but little from that of N. 45° E., which M. de Humboldt, at the beginning of this century, pointed out in the slaty rocks of the coast of Venezuela, and in the mountains of granitic gneiss, from the lower Orinoco to the basin of the Rio-Negro and of the Amazon.*

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* *Vide* the Rapport on the Memoir of M. Pissis, Comptes rendus, t. xvii. p. 28.

Nevertheless, the hills of gneiss which occur in the Pampas between Cape Corientes and the Sierra of Tapalquen, as well as the hills of Monte Video, are characterised by a different direction, running from W. 25° to 30° N., to E. 25° to 30° S. M. D'Orbigny gives them the provisional name of *Pampean system*, and he thinks that this system is nearly as ancient as the *Brazilian system.* If subsequent observations confirm this conjecture, the relations of these two systems, whose directions are nearly perpendicular to each other, will naturally recal the relations which subsist in Europe between the Westmoreland system and the system of the *Ballons.*

In the midst of the multitude of dislocations of which the Silurian series presents traces, M. D'Orbigny has endeavoured to ascertain the *soulevements* which affected this system before it was covered by subsequent formations, but he has not been able to define any one of them with certainty. He has not succeeded better with the Devonian system, for a most attentive examination of the innumerable multitude of mountains and hills belonging to this series has not enabled him to discover any system of dislocations specially limited to itself; but in Brazil M. Pissis has pointed out a system of dislocation which he regards as immediately posterior to the formation of the transition series, "whose deposition," he says, "was interrupted by commotions which elevated it at some parts to a height of a thousand or eleven hundred yards above the level of the sea, and produced at other points large fissures running east and west, through which escaped diorites, that spread themselves like lavas, and modified the rocks they met with. The most elevated mountains of Brazil, those of the province of Minas Geraës, viz. Itacolumi, Caraca, Morro Itampe, and the plateaus to the south of San-Paolo, belong to this *soulèvement*, which gave the beds an EW. direction, and communicated to the surface its present form."*

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* Humboldt's Essai Géognostique sur le Gisement des roches dans les deux hémisphères, p. 56.

M. D'Orbigny terms the whole of the ridges formed by this dislocation *Itacolumian system.* He would be induced to connect with it the mountains of the Malouine Islands, which he designates by the name of the *Malouinian system*, if it should be ascertained that these mountains are formed of Silurian beds having an EW. direction.

Thus, according to him, the gneiss islands, which form the most ancient portion of the American surface, were extended towards the west by dislocations, which took place after the deposition of the transition formations, while, perhaps, new points were elevated from the bosom of the waters in the Malouine Islands, and near Cochabamba, in Bolivia. This phenomenon appears to have been anterior to the deposition of the carboniferous system, subsequently to which new dislocations occurred, whose most marked traces were observed by M. D'Orbigny in the province of Chiquitos.

The hills of this province have gneiss for a fundamental rock, on which repose silurian and devonian beds; and these are covered by sandstones referred by M. D'Orbigny to the upper layers of the carboniferous system, and flanked by triassic beds, and by tertiary deposits. These hills present a general parallelism which renders them a well characterised system, having a direction from ESE. to WNW., and to which belong the chains of Parecys, of Diamantino, and of Cuyoba, in the western portion of Brazil. M. D'Orbigny terms the whole the *Chiquitian system*, and considers it as posterior to the last carboniferous layers, and as anterior to the trias, seeing that the last beds which are seen to be deranged belong, according to him, to the carboniferous system.

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* Pissis, Comptes rendus des Séances de l'Académie des Sciences, t. xiv. p. 1044.

The production of a great system of dislocations in South America at this epoch, is confirmed, according to M. D'Orbigny, by the immediate contact of the variegated clays of the region situated to the east of Cochabamba, with the devonian formations. This contact seems to announce a denudation of the carboniferous formations anterior to the deposition of the triassic series.

The hills of the Chiquitian system nearly join the mountains of Brazil at the base of the Andes. We have thus a new appendage added to that already formed by the Itacolumian system. When we cast our eyes over the geological map of Bolivia prepared by M. D'Orbigny, it may appear at the first glance that there are numerous features of resemblance in the arrangement of the formations of the hills of Chiquitos, and of the eastern chain of the Andes. However, the direction which predominates in the mountains of Chiquitos is not exactly the same as that of the ridges occurring on the flanks of the Cordillera, to the south-east of the plains of Moxos and of Santa-Cruz-de-la-Sierra, and the height of the two masses of mountains is too different to make it natural to suppose that they should be referred to one and the same epoch of soulèvement.

The colossal mountains which rise above the north-east of the lake of Titicaca, and with which is connected the whole eastern region of the Cordilleras, from the fifth to the twentieth degree of south latitude, or, to speak more correctly, the Andes properly so called, the *Antis* of the ancient Incas, form a distinct system, to which M. D'Orbigny has given the name of *Bolivian system*. The mean direction of this system, very different from the directions which prevail in the rest of the Cordilleras, is from SE. to NW. The ridges composing it consist of elevated beds of the silurian, devonian, carboniferous, and triassic formations. This celebrated Nevados of Illimani and of Sorata, ascertained by Mr Pentland to be the most elevated summits of the New World, are the two culminating points of an axis of granatoidal rocks, running from SE. to NW., and which having doubtless been elevated through a great *crevasse*, has been the cause of the elevation of the whole Bolivian system.

This elevation took place after the deposition of the trias, as is shewn by the triassic beds which M. D'Orbigny has seen in an inclined position, and at a height of more than 4000 yards above the sea. The triassic formations form the last upraised beds in the different localities where they have been observed in Bolivia. At all the parts of the Bolivian system where M. D'Orbigny has seen them, when they are covered, they are so only by horizontal beds of the Pampean formations, or by modern alluvia, products which are entirely terrestrial, and not marine. It thus appears certain that the Bolivian system has assumed the characteristic features of its outline, subsequently to the period of the triassic formations. We may also conjecture that this phenomenon took place before the deposition of the jurassic and cretaceous formations, for otherwise these formations would have been deposited on the trias of Bolivia, and would have been upraised along with it.

Probably, therefore, it was between the triassic and the jurassic periods, or nearly at that epoch of our European chronology, that the whole mass comprised between the western *plateau* of Bolivia and the plains of Santa-Cruz and Moxos was elevated above the ocean.

Endeavouring to complete, at least in a conjectural manner, the *tableau* of the great geological phenomena of which South America has been the theatre and the product, M. D'Orbigny is led to suppose, from the observations of the latest travellers, that two great dislocations took place during the great cretaceous period: the one, represented by the *Columbian system*, running from N. 33° E. to S. 33° W., formed the mountains of Suma-paz and of Quindiu, elevating the cretaceous formations of the *plateau* of Bagota; the other produced the *Fuegian* system, which occupies the western portion of Terra del Fuego, and runs from N. 30° W. to S. 30° E.

The effect of these different and successive phenomena must have been to elevate above the ocean the principal mountainous centres of South America; but these different groups could not then have been yet connected together by the great continuous chain of the Cordilleras. This vast chain is sinuous, like our Alps. It presents different portions which are very variously disposed: without speaking of those which M. D'Orbigny refers to the Columbian and the Fuegian systems, and without leaving the region examined by himself, two very distinct directions are observed. From the Straits of Magellan to Bolivia, over a space of 35°, which embraces the whole length of Chili, the Cordillera runs from S. 5°. W. to N. 5° E.; afterwards, in Bolivia itself, it makes a sudden bend to the west, and runs from SE. to NW. On entering South Peru, the mountains preserve a constant parallelism to those of Bolivia, as far as the fifth parallel of south latitude; a fact which permits us to suppose that the geological lines observed by M. D'Orbigny in the Bolivian system, to the east of the Cordillera, properly so called, extend as far as that latitude, thus embracing a total extent of 15°. Further north, the chain again changes its direction, and takes that of the Cordillera of Chili.

Thus, in the region comprised between the Straits of Magellan and the equator, the Andes present two great systems of mountain-chains and valleys. These two systems, which M. D'Orbigny designates the *Bolivian* system and the *Chilian* system, cross each other nearly in the same manner as the systems of the Western Alps and of the principal chain of the Alps cross in Europe, and they appear likewise to be the result of successive dislocations.

The circumstance that the Cordillera, in the region between Terra-del-Fuego and Quito, is composed of several large fragments variously placed, and probably of different origins, is connected with a curious fact, which confirms, in a remarkable manner, the reality of the distinction founded on the difference of directions. No earthquake has ever been felt on the great Bolivian *plateau*. This, at least, was what M. D'Orbigny was informed, and it corresponds with his experience in the latitude of Arica. It is natural to ask, if the presence of the Bolivian system in this latitude has not some influence connected with the limitation of earthquakes. It appears, in fact, that very violent shocks are experienced in the centre of the

Cordillera of Chili, simultaneously with the earthquakes which ravage the coast, near to which they take place with their maximum of intensity.

Another peculiarity which distinguishes the Chilian system from the Bolivian system, is the presence of patches, which are still problematical, of the jurassic formation, and of largely developed masses of the cretaceous formation, in beds much dislocated and raised to great heights. According to M. D'Orbigny, it was after the cretaceous period, but before the period of the tertiary deposits, that the Chilian system had its origin. It was produced by the eruption of the porphyritic rocks, or perhaps only of a portion of these rocks, which, in South America, are of various kinds. M. D'Orbigny found at Cobija on the coast of the Pacific, sienitic porphyries of a blackish colour and very compact; at the Morro of Arica, pyroxenic porphyries; at Palca (Bolivia), and at Machacamarca, sienitic porphyries; in the mountains of Cobija and of Palca (Peru), and throughout the whole western line of the Cordilleras, he found a great variety of old amygdaloidal wackes, containing a large quantity of different substances; at the Missions, there is a greyish or violet-coloured amygdaloidal rock. Porphyritic rocks have also been observed by Messrs Gay, Darwin, and Domeyko, in different parts of the Cordillera of Chili.

According to M. D'Orbigny, the termination of the cretaceous period was marked in South America by a series of dislocations, which occurred to the west of the land already projecting from the sea, and which gave to the Cordillera of Chili its first relief, by permitting the elevation of a continuous series of porphyritic masses. This vast porphyritic eruption took place in the direction from N. 5° E. to 5° W. between the Straits of Magellan and the junction of the Chilian system with the Bolivian system, which the band of eruptive rock has accompanied to the west, elevating the cretaceous formation of the *plateau* of Guancavelica. The violent commotion of the water caused by this movement had, as its result, according to M. D'Orbigny, the formation of the Guaranian tertiary deposit, which covers the province of Moxos, and which seems to be spread over the bottom of a large portion of the basin of the Pampas. There is thus attributed to this deposit an origin analogous to that which has often been attributed in Europe to a portion of the plastic clay formation. The absence of fossils in the Guaranian deposit, its invariable ferruginous nature, and its imperfect stratification, would seem favourable to this supposition.

A new period of repose having then succeeded to the disturbances, the tertiary seas extended to the east and to the west of the Chilian system. The marine sedimentary deposits of the Patagonian formation began to extend over the Guaranian formation. Terrestrial streams transported from the neighbouring continents bones of mammifera, fragments of wood, and fluviatile shells; some of them coming, no doubt, from the ridge of the Chilian system, would convey from the SE. bones, still provided with their ligaments, into the Patagonian sea, while others arrived from the great northern continent, that is to say, from Brazil, which had already emerged in a great measure from the ocean. The continent of South America already possessed, so to speak, in the state of outline, the configuration which it was to preserve; it already presented a chain rising above the ocean, indicating the course of the Cordillera from N. to S., and thus separating the Atlantic Ocean from the Pacific Ocean by a narrow tract of land, similar to the isthmus of Panama of the present day. We can thus conceive how the tertiary formation of the two sides may have been contemporaneous, although they do not contain species of fossil shells common to both; and, notwithstanding the reservations which we have made above, it must be confessed that the hypothesis proposed by M. D'Orbigny, explains so happily the complete difference of the Faunas of these two formations, of at least a nearly similar age, that it is difficult not to regard it as possessing a very considerable degree of probability.

But the seas which then encroached so largely on the form ultimately assumed by South America, receded and removed from the base of the Cordillera, leaving the continent to increase in size, towards the east, by the amount of all the space occupied by the Patagonian tertiary formation, and towards the west by the tract occupied by the tertiary formation of Chili, which runs along the whole extent of the Chilian Cordillera.

M. D'Orbigny connects this event with the appearance of the trachytes which were erupted in the axis of this Cordillera, and which completed its relief at a very modern epoch.

By studying the position of the trachytes, and of the trachytic conglomerates, M. D'Orbigny has been able to convince himself that these two species of rocks have performed very different parts. His maps shew, in fact, that the solid trachytes must have risen in an incandescent state, at different times, over great lines. Sometimes elevated in pasty masses nearly solid, they have given rise to those obtuse cones so remarkable, and at the same time so characteristic, which, at the summit of the Cordilleras, have absolutely the same form as in Auvergne. If at other points these rocks have a stratified appearance, this evidently results from the eruption of more or less liquid masses, which have spread themselves out in sheets. Of this we have an example in the section left by the Rio Maure, where the author distinctly remarked the alternation of masses of trachyte with pumice conglomerates; and also on the coast near Tacna, where the pumice conglomerates cover thin layers of trachytes. With the exception of the alternation observed near the Rio Maure, M. D'Orbigny has always found the trachytes under the conglomerates. The former present very various asperities, while the latter everywhere form masses like beds, nearly horizontal, which level these asperities. The pumice conglomerates are composed of alternate beds of pumice, more or less considerable, or of fragments of obsidian, and the component ingredients are not united by any kind of cement, a circumstance which leads us to believe that the conglomerates have been ejected in the state of cinders, during the eruption of the trachytes, or posterior to it. We may even ask if all the conglomerates belong to the same epoch as the trachytes, and if their superior position does not shew that they sometimes belong to a more modern period.

In South America, the trachytic rocks only shew themselves in the chain of the Cordilleras, and there most frequently accompany porphyritic rocks. In Bolivia, they only present themselves on the Great Bolivian *Plateau*, on the Western *Plateau*, and on the western side of the Cordillera. No one has observed them in Brazil.

M. D'Orbigny admits that, on the western side of the long ridge which formed the first outline of the Cordillera, and consisted of elements belonging to the different systems mentioned above, a new opening took place, and incandescent trachytic matters, pushed with violence towards this vast outlet, escaped in all directions, dislocated the porphyries and the cretaceous rocks, and invaded the whole summit of the chain.

In the immense mass of Bolivia, events apparently more complicated took place. The lines of dislocation of the Chilian system meeting with *reliefs* existing previously to the Bolivian system, and not being able to fracture this great mass, extended to the west, as the porphyritic rocks had done previously. The trachytes and their conglomerates, which, according to M. De Humboldt, form an immense dome on the *Plateau* of Quito, formed, according to M. D'Orbigny, another dome on the Western *Plateau* of Bolivia. Moreover, these rocks issued through fissures in the sedimentary rocks, along that line, so interrupted by trachytic eminences, which, to the east of the Great Bolivian *Plateau*, borders the foot of the dislocations of the Devonian rocks from Achacoche to Potosi. They are not the primary cause of the Bolivian system, but they upraised some portions of it, while, at the same time, they perhaps communicated to the Chilian Cordillera the greater part of its external shape. The trachytes thus acted in the New World as in Southern Italy and in Greece, where their lines of eruption followed those of systems of mountains of more ancient origin, especially of the system of the Pyrenees.

A dislocation of 50°, or upwards of 3400 English miles in length, which produced one of the highest chains in the world, and which elevated above the ocean all the marine tertiary formations of the Pampas over an immense extent, could not have taken place without causing a proportional displacement in the waters of the sea. It was then, according to M. D'Orbigny, that the latter, being agitated with violence, invaded the continent, destroyed and transported the great terrestrial animals of the extinct fauna, such as the *Mylodon*, the *Megalonyx*, the *Megatherium*, the *Platonyx*, the *Toxodon*, and the *Mastodon*, depositing them, along with earthy matters, at all heights, in the terrestrial basins or in the neighbouring seas. Those matters simultaneously transported and deposited on the *Plateaux* of the Cordilleras to a height of 13,000 feet above the ocean, on the plains of Moxos and Chiquitos, and over the whole bottom of the great basin of the Pampas, constituted the Pampean formation. This Pampean formation, which occurs in horizontal beds at all heights, which is everywhere composed of the same loams, and which only contains remains of mammifera, could only have been produced by a general terrestrial cause. M. D'Orbigny believes that he has discovered this cause in one of the soulèvements which took place in the Great Cordillera, and which must have produced a sudden displacement of the waters of the ocean; which latter, moved and agitated with violence, invaded the continents, and destroyed the great terrestrial animals, transporting them tumultuously to the lowest portions of the continents, or into the bosom of the deep; and it is evidently to the soulèvement of the trachytes alone that the phenomenon can be attributed.

M. D'Orbigny has remarked that, at some points on the Bolivian *Plateau*, the trachytic conglomerates appear to cover the Pampean formation, a circumstance which would lead to the belief that they are posterior to that great deposit. This observation coincides with the one noticed above, viz. that the trachytic conglomerates do not appear to be all exactly of the same age. The greater part are contemporaneous with the Pampean formation, but some of them are posterior. In Auvergne, the numerous mammifera of the fauna anterior to that epoch, and which are found at various localities, are enveloped by trachytic rocks and their conglomerates. We have here an approximation which may not be without its value.

To this movement may perhaps be referred or compared many facts observed in various parts of the surface of the globe, since we everywhere meet with remains of a terrestrial fauna, now entirely extinct; and because, in a multitude of localities, we find deposits analogous to those of the Pampas, containing bones of the mammifera, belonging to species which have been destroyed.

The appearance of the trachytic rocks of which the highest summits of the Cordilleras of Chili and Peru are composed, does not, however, seem to have been the last of the great geological movements of which South America has been the theatre. This eruption seems to be connected with the origin of the Pampean loam, and this formation is covered, as we have seen above, by other deposits, which indicate another and more modern great event. This last great event can only be sought for in the first outburst of the American volcanoes now in activity, which, up to the period just mentioned, had not yet commenced the series of their eruptions. The long line of the volcanoes of Chili, ranged in accordance with the axis of the trachytic zone, is the extreme link of that great zig-zag volcanic chain, which, resting on the half of a great circle of the earth, described from the republic of Bolivia to the Birman empire, marks the limits of the great mass of the American and Asiatic Continents, and of the vast oceanic extent of the Pacific. It was, without doubt, a memorable day in the history of the inhabitants of the globe, and perhaps even in the history of the human race, when that immense volcanic battery, which does not include less than two hundred and seventy principal orifices, was opened for the first time. ***Perhaps the traditions of a universal deluge are connected with this great event, which could not fail to be a fearful disaster.*** The author is favourable to this opinion, which had already been previously proposed, but only as an hypothesis. He adduces, in support of it, many facts which, even although they should remain isolated, seem to us deserving of the attention of geologists.

We have already quoted the observations according to which M. D'Orbigny concludes that the recent elevated shells on the shores of the Atlantic and the Pacific could not have been raised by a slow action, but by a sudden movement. These remarks, together with the facts also noticed in relation to the beds of conchillas of the Pampas, to the shells of Monte Video and of Patagonia, and to all those of the coast of the

Pacific, lead him to admit a sudden general elevation of the whole coast, which gave rise to the present configuration of the Continent. This last movement of the American surface, which coincided with the first outburst of the volcanoes, would produce a commotion in the adjacent seas, whose waters rising above the crests of the mountains, hollowed them out, broke up the surface at all heights, and transported vast masses of alluvial matter into the plains.

The traditions of a deluge, which have been met with among most of the American nations, may be only a souvenir of this last revolution. The discovery made by M. D'Orbigny of the remains of human industry in the alluvium of the plains of Moxos, on the banks of the Rio Securi, is an additional reason for this conjecture. As it is at least evident that that event was posterior to the existence of the present marine fauna, M. D'Orbigny has considered himself entitled to term its products *diluvial formations.*

In conclusion, it results from the investigations of M. D'Orbigny, that the new continent has been formed, like the old one, by the successive *soulevements* of the different systems of mountains which traverse the surface; that these systems become more and more extended in proportion as their origin approaches the present period; and that the *reliefs* resulting from these different systems have been successively added to one another, advancing generally from the east to the west. Thus the most ancient prominences presented by the American Continent appear to have had their origin in the eastern region of Brazil, after the epoch of the formation of the gneiss. The transition formations next made their appearance to the west, and increased the original continent by the amount of the whole Itacolumian system. The carboniferous formations, to the west of the two others, form a part of a new appendage composed of the Chiquitian system. The triassic formations, to the west of the three first systems have been upraised in the Bolivian system, a surface of much greater extent than the others. Up to that period America had been elongated from the east to the west. The cretaceous formations ceased to be deposited, and the Cordillera, still to the west of the land already elevated, was the first to assume a *relief* from north to south, thus changing completely the form of the Continent. Subsequently the eruption of the trachytes, and the first outbreak of the now existing volcanoes, completed the external forms of this vast chain, and gave to the shores of the Continent their present configuration; and it is very remarkable that these last phenomena manifested themselves more especially in the western region of the Continent, where the earthquakes of the present day have concentrated their action.

This general remark on the advance of the *soulevements* from the east to the west, leads to a curious analogy between the New and the Old World. Buffon had already been struck by the difference in the form of the two great Continents. He had remarked that in the Old Continent, or to speak more exactly, in Europe, Asia, and the north of Africa, the great geographical features are arranged in relation to an east and west line, nearly as they are in the New World in relation to a north and south line. Mr Poulet Scrope, in addition to this observation of Buffon, remarked the essential difference presented by the east and west sides of the South American Continent, in that, while the one presents a long ridge bristling with peaks and volcanoes, the other exhibits large rounded mountains, without any indication of volcanic phenomena. The results obtained by M. D'Orbigny enable us to characterise this analogy more exactly, inasmuch as it appears that in South America the successive *soulevements* which have fashioned the *relief* of the Continent, had generally their principal point of application more and more to the west in proportion as they are more modern; whereas in Europe the *soulevements*, in proportion as they are more modern, exercised their principal effects more and more to the south.

In America the great plains of the Pampas and of the Amazon, correspond to that great plain in the north of Europe, of which a small depression is occupied by the waters of the Baltic; and the vast lake of Titicaca fills the sinuosities produced by the meeting of the various systems which cross one another in the Andes, much in the same manner as the Mediterranean fills the larger and deeper sinuosities caused by the crossing of the system of the Pyrenees, the Alpine systems, and some other modern systems.

The two Continents present each a great exception to the rule indicated relatively to the direction in which the *soulevements* have succeeded one another. The one is in the modern dislocations which, according to the observations of M. Pissis, have given rise to the external form of the eastern coast of Brazil; the other is in the presumed modern *soulevement* of the great line of the Scandinavian Alps: but the existence of corresponding exceptions both in the one and the other, forms an additional analogy, and this analogy is so much the more curious, because the two chains which constitute the exception, belong to one and the same system of mountains, the system of the Western Alps.

Comparisons analogous to those which we have just been making between Europe and South America, had already been established between Italy and India, and between Europe and North America; the investigations of M. D'Orbigny will contribute to render these comparisons less rare and more easy. They will even present a point of departure more elementary than those upon which science has hitherto been able to rest. We believe that there is much justice in the following remark made by M. D'Orbigny, towards the conclusion of his Memoir, on the small degree of complexity of South America. He says, "Owing to the extreme simplicity of its geological composition, and owing to the large proportions of each epoch, South America is perhaps, of all parts of the globe, the most easy to understand geologically, and that where study is destined to throw the greatest light on the great revolutions to which our planet has been subjected. Far from being, like Europe, subdivided into a great number of patches of formations, or traversed by innumerable transverse chains, whose epoch it is difficult to determine with precision, South America presents *reliefs* extending over hundreds of leagues, and deposits stretching over several degrees of surface. There, every thing is exhibited on a great scale, the mountains as well as the basins, and on that great Continent every thing is visible—the powerful causes and their vast effects."

On the Classification of Fishes. By L. Agassiz.

Far from participating in the opinion of those who regard our classifications merely as an artificial scaffolding calculated to facilitate our researches, by assisting the memory, I am firmly persuaded that the progress of the natural sciences will, sooner or later, lead to the establishment of a system which shall be the true and complete expression of the various relations by which the entire series of created beings are connected with each other. But such a system cannot be established until we acquire a more complete acquaintance with the innumerable variety of objects in this vast field of inquiry. The attempts hitherto made to attain to it, appear to me to be only the first foundation of the edifice, a provisional means for recognising when we are among the varied forms which must be registered according to their diverse affinities. Even the very principles which must guide us in this operation have not been definitively settled. We may compare the efforts of naturalists desirous of grouping natural bodies in the most convenient manner, to the labours of engineers wishing to represent the aspect of a country in a map. They first fix a few salient points from which they can command the whole. From thence they enter upon the details by subdividing the considerable extent comprised within their first triangulation; they then study each new section by itself, by traversing it in every direction. It is then only that they can begin to note the peculiarities which form the special character of these restricted compartments. With such materials alone a good map may be constructed. But one observer would wish further to learn the heights of the mountains which are indicated in relief; another would desire to know what parts of the surface are wooded, or what is cultivated, and what not; while another might desire information on the different climatological phenomena, and find nothing to guide him. In these respects, therefore, a map constructed on the basis mentioned, would be insufficient for the wants of science. New researches would become necessary; the results of geological and meteorological studies would have to be combined with geodesical details; drawings to represent the accidents of the formations would have to be completed; and the design would have to represent the relief itself as much as possible. Then the time may come, perhaps, when the requirements of science will go so far as to render, in most instances, real reliefs indispensable; that is to say, the material reproduction of forms, reduced to certain dimensions, will one day become a necessary accompaniment to topographical works.

May we not say the same thing of systems in natural history? There was a time when vague approaches were sufficient to give an idea of the limited number of beings, imperfectly known, which constituted the subject of the naturalist's study. They were grouped according to some conspicuous characters, easily perceived; sometimes all that was attempted was to place them after one another, according to their size, their manner of life, or the places which they inhabited. However incomplete these methods were, they still satisfied the wants of inquirers at that period; and notwithstanding their imperfection, they even contributed to the progress of the natural sciences. Some author or other, by remarking the gaps in such arrangements, completed the method; others collected new materials calculated to facilitate the researches of their successors, and by degrees new systems arose, founded on good characters. From that period the progress was rapid; monographical works came to extend the field of comparisons, by fixing new bases for the study of details. New ideas led to the discovery of new aspects in subjects supposed to have been exhausted. It was thus, without leaving the domain of zoology, and without going back to the first attempts at classification proposed for the animal kingdom, that naturalists confined themselves for a long time to seek for the distinctive characters of species, and to group them in a small number of genera, often founded on a very imperfect acquaintance with their organisation. This tendency is particularly characteristic of the works belonging to the school of Linnæus, which caused the science to make immense progress, by simplifying the method and limiting it to the most concise expression of known facts. It was soon perceived, however, that this system could be regarded only as a frame-work fitted to include, in one view, all the classes, but the compartments of which were poorly furnished. Every day, in truth, enriched science with important facts, which outgrew, beyond measure, the limits assigned by Linnæus to the extent of his incomparable diagnoses. Comparative anatomy, in particular, by investigating the internal structure of animals to the minutest details, furnished to zoology more precise characters for fixing the limits of classes, orders, and families. In place of simple diagnosis, attempts were thenceforth made to form descriptive pictures of the entire characters of all the natural sections which could be circumscribed in a precise manner; naturalists endeavoured to arrange the characters according to their relative value in the functions of life; species were strictly compared with each other; all the facts relating to their manner of life, their reproduction, and geographical distribution, were carefully registered. It is to the immense influence which the works of Cuvier have exercised on the development of the natural sciences, that this new direction given to zoological studies has principally to be ascribed; and it may be affirmed that it is in this same spirit that most of the great monographical works which have been continually enriching science for the last quarter of a century, have been conducted. There are few classes which are without their monographs: the facts of structure which have been studied and the species examined, are now in general represented with so much exactness, that we can form an accurate idea of them without ever having seen them in nature. Such a detailed knowledge of species, and such multiplied researches into the organisation of the principal types of all classes of the animal kingdom, must necessarily bring about great changes in classification. Accordingly, we have seen systems multiplied without end. Yet, notwithstanding their number, they do not differ essentially from each other, and in all of them we can more or less recognise the influence of Cuvier's works; the differences which distinguish them consist principally in the respective position of the great divisions relatively to each other, resulting from the different principles which guided their authors, and the extension assigned to these same divisions; for it will be understood that we cannot regard as particular systems all the systematic sketches, in which, for the most part, there is nothing original, and the outlines of which differ only in the order in which groups succeed each other, and in the names applied to them. The most important changes effected in the general system of zoology, since the time of Linnæus, consist in the dismemberment Cuvier has made of the Swedish naturalists' shapeless and undigested class of Vermes; and it may be affirmed, without in any degree depreciating the value of the works of modern naturalists, that they are only a development of the primary sections of the great French naturalist. The modifications which these classifications have been subjected to in detail, do not appear to me less important; but they belong to so many different authors, that I cannot here undertake to give any account of them; I shall only say that they have essentially borne upon defining the limits of families and genera, and upon a more complete and rigorous appreciation of their characters.

But while this advancement was going on in zoology properly so called, a new science arose, under the hands of the same individual who had already contributed so powerfully to the development of zoology. The study of fossils acquired, from the profound researches of Cuvier, an importance hitherto unknown to it, from the time when he demonstrated that the remains of organized beings embedded in the strata of the earth, are generally different from the living species, and even belong to different generic types. This fact having been firmly established in regard to the mammifera and reptiles, investigations were increased in all the classes, and in relation to all the series of strata composing the solid crust of our globe in which fossils are found. The relations of these primitive beings with those which at present people the surface of the earth, were inquired into; observers were desirous to appreciate their analogy, and determine the differences which distinguished them. This investigation was the cause of new and great progress in zoology and the comparative anatomy of the solid parts of the bodies of animals; and it is easy to see that the influence of paleontology on zoological and anatomical studies will become more and more important in proportion as these different branches of science become more closely united. I do not even doubt but that we shall soon be led to unite the results of panleontological and zoological researches into one body, as soon as an attempt is made to establish a complete system of natural affinities throughout the entire animal kingdom. The lacunæ are in fact too obvious and too numerous, when fossils are not taken into account, to admit of zoologists for the future dispensing with the enumeration of them along with the living species, in their attempts at classification.* For, by omitting them, we obtain only the fragments of the frame-work, and can attain only to an incomplete exposition of the plan followed in the creation of organized beings. We have long been assured of the fact that the beings which have disappeared from the surface of the globe, far from having lived simultaneously, succeeded each other at different epochs, and have belonged to different creations, or rather that they have constituted series by themselves which have had a limited existence, and been replaced by others after longer or shorter intervals. Hence arise new requirements for systematic zoology. It will not be sufficient henceforward to group genera and species according to their organic affinities; we must also take into account the relative age of their appearance on the surface of the globe, and the importance of each group in the different epochs of the general development; in a word, zoology ought to comprehend in its systems the genealogy of the whole animal kingdom.

Important works have already pointed out the relations which exist between the natural affinities of the genera and species of many families, as well as their geological age; but perhaps no class exists in which this succession of types, and their relations with the geological formations to which they belong, is more evident than among fishes. It may, indeed, be affirmed, that the closest connection exists between the principal types of this class, and the epoch of their progressive development. We have only to glance at the tables of species characteristic of the formations, which I published at the end of vols. 2, 3, 4, and 5 of my work,† to be convinced that each order, and even each family, follows a particular progression; that there is, in regard to each group, a beginning and an *apogée* in its development; that by turns they terminate by becoming extinct, if they go back to a remote antiquity, or by acquiring a considerable extension in the present creation, if their appearance dates only from a recent epoch. These results, which are so evident in the class of fishes, I have, in like manner, established in that of Echinodermata; and although I have not yet given a detailed explanation of the general results of my studies respecting these animals, I can yet affirm that I have recognised among them the same laws of development. To be convinced of this, it is enough to remember in what proportion we find the Crinoïdes and star-fish in the series of formations, and what is the condition, in narrower limits, of the different families of the order of Echinites. With such results before us, we are naturally led to suppose that it is the same with the other classes of the animal kingdom; and that, if we have not yet succeeded in seizing everywhere the thread of their development, it is because we have not found out the key to their connection. We already possess, in regard to all the classes, positive indications of this preponderance, at determinate epochs, of certain types, which change proportion with their cotemporaries belonging to more recent eras; for example, among the Mammifera, the Pachydermata, the Edentata, the Marsupialia, and the Quadrumana; among reptiles, the Ichthyosaures, the Plesiosaures, the Megalosaures, the Ophidians, and the tailless Batracians; among the Crustacea, the Trilobites; among the Cephalopoda, the genera with partitioned shells, whose development is most remarkable, from the Orthoceratites and the Goniatites, down to the singularly plicate enrolled or straight forms of the Scaphites, the Ancyloceras, the Cyrtoceres, the Ptychoceres, the Turrilites, the Helicoceres, and the Baculites. Among the Acephales, may we not point out facts in every way similar between the Brachiopodes and the Lamellibranches? And is it not a very significant fact, that we observe this regularity in progressive development shew itself in a manner so much the more evident, as we endeavour to find the marks of it in the best known classes? From this consideration, very powerful arguments may be advanced against the objections which some are desirous to draw from the imperfect state of our knowledge respecting the entire series of fossils embedded in the strata of the whole globe. But it is evident, that the knowledge we have already acquired in this respect, ought to have an influence on our classifications; and that authors will thus come always to take more into account the order of the succession of types in their systematic arrangements.

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* In my monographs of living and fossil Echinodermes, I have endeavoured to realise in certain groups, still very few in number, it is true, this idea of a union of zoology with paleontology and comparative anatomy. It is much to be wished that similar attempts were made with reference to all the classes of the animal kingdom.

† Recherches sur les Poissons par L'Agassiz. Qto,

I have already had more than one occasion to draw attention to the striking analogy which exists between certain embryonic forms, which are transitory in the development of individuals, and the constant characters of numerous genera belonging to different families, which have but few representatives in the existing creation, or have become wholly extinct. It cannot be doubted, therefore, that these considerations ought to exercise, in their turn, an influence on the position to be assigned, in the system, to these same genera. When resuming my researches on the conformation of the skeleton of fishes, I have shewn, at different times, how far the results of embryology agree with those of paleontology. I have thence become convinced that embryological researches, prosecuted with the view of appreciating the value of organic forms, as zoological characters, ought, in like manner, one day to exercise a great influence on our methods. It will, no doubt, be the same with microscopic investigations, which are now pursued with so much ardour in every branch of natural science.

Do the relations between organized beings, thus varied as they are, admit of being expressed by linear series? I think not. I am more inclined to believe that naturalists will revert to the idea of well-defined divisions, placed after each other, to admit, as the expression of the varied relations of organized beings, of graphic pictures, in the centre of which the best known types will be placed, and around which will be ranged, according to their greater or less affinity, other types, which may become, in their turn, the centre around which other secondary types will gravitate. And the better we become acquainted with the entire details of one great division, the better will we group all its members, according to their diverse affinities. If we are considering the Echinodermata, for example, it will be of importance to notice how this class is connected with the Vermes by certain genera of the order of the Holothurias, and with the Polypes by means of the Crinoïdes. If we wish to divide the Crinoïdes in the most natural way, we must insist on the analogy of the Echinocrines, for example, with the true Echinides, and on that of the Comatules with the Asteriæ, while the true Encrines will form the central type of the order; and so on in succession. And in order to combine the indications relating to the affinities of one class with those which we possess respecting their succession, it will be necessary to add to these zoological charts, as they may be called, genealogical trees, on the trunk of which the most ancient genera will be inscribed, while the branches will bear the names of the most recent types. By properly managing the proportions of the trunk and branches, and making them of suitable dimensions, we may even indicate exactly the period when each group appeared, by giving to the different branches of each order a degree of thickness proportionate to the importance of the part which the types they represent have occupied in each geological formation.

It is in accordance with these principles that I have constructed the annexed table, which represents the history of the development of the class of fishes across all the geological formations, and which expresses, at the same time, the degrees of affinity which the different families have to each other.* At the top of the figure are inscribed the names of the four orders into which I divide this class, and the characters of which are discussed in the *Recherches sur les Poissons Fossiles.* These are, the orders *Cycloïdes*, *Ctenoïdes*, *Ganoïdes*, and *Placoïdes.* Below these appear the names of the families which have representatives in the presently existing creation. They are arranged vertically, to correspond to the ascending lines, more or less strongly marked, which indicate, by their lower extremity, the point of departure in the development of the families, and, by their breadth, the degree of importance which they possessed at each epoch. On the sides of the table are inscribed the names of the principal formations, in order to indicate the geological levels from which all the families take their origin, as well as those to which they rise. The names of the families which do not come down to the present creation are inscribed on the trunks which represent them; those which have no fossil representatives are simply indicated by broad lines on the level which denotes the present creation. Finally, the convergence of all these vertical lines indicates the affinity of the families with the principal trunk of each order. I have not, however, connected the lateral branches with the principal trunks, because I am convinced that they do not descend the one from the other, by way of direct procreation, or successive transformation, but that they are materially independent of each other, although forming an integral part of one systematic whole, the connection of which cannot be traced but by the creative intelligence of its Author. Having ascertained that the species of each formation are always different from those of other epochs, I have drawn lines of demarcation from the geological levels, across all the ascending lines of the families, in order to shew that the genealogical development of the species is often interrupted, and that if, notwithstanding, each trunk shews a regular progression, this filiation is not the result of a continued descent, but rather a repeated manifestation of an order of things determined beforehand, tending towards a precise object, and methodically realised in the order of time. I have not pretended to express in a limited synoptical table, of a class so numerous as that of fishes, all the facts I have studied, and what I could have developed in this place, even to an enumeration of all the species. I have only wished to present a sketch, which might express the general idea of which my whole work may be regarded as a detailed exposition, and which a glance at the figure will render easily understood. Two orders of the class appear alone, appertaining to the earliest periods of the developement of life on the surface of the globe: they appear there simultaneously with the representatives of all the classes of invertebrate animals, while they continue for a long period the only existing types of vertebrate animals. The principal development of these two orders, namely, the Ganoïdes and the Placoïdes, takes place in the formations anterior to the chalk, and their typical families become extinct before the present creation, where they are represented only by a few species; such are, in the order of the

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* The Table will be given in our next number.

Placoïdes, the *Cestraciontes*, and the *Hybodontes*, with their subdivisions; and among the Ganoïdes, the *Lepidoïdes*, the *Sauroïdes*, the *Celacanthes*, and the *Pycnodontes*, along with the less important groups, and *Cephalaspides*, *Dipteriens*, and *Acanthodiens*. The collateral brinks of the Placoïdes, which are in general poorly represented in the existing creation, terminate rather early; the *Squalides* commence in the coal formation, the *Chimeras* and the *Rays* soon after. The *Cyclostomes* are the only kinds that exist exclusively in our own times. But at the epoch of the chalk, every thing is changed in the class of fishes. All of a sudden we perceive two orders appear, the *Ctenoïdes* and the *Cycloïdes*, as much diversified from their origin as their predecessors were. Before the commencement of the tertiary period, the Ctenoïdes comprehend nine distinct families, to which two others must be added during the tertiary epoch, and at the commencement of the present era. The Cycloïdes are more diversified still; for after they appear, the type of the Acanthopterygians presents itself by the side of the Malacopterygians, and their numerous families ascend, for the most part, into the era of the chalk. But, notwithstanding these differences, there is a close analogy between the primary representatives of all these types. During this period, the Placoïdes are reduced, so to speak, to the families of the Chimeræ, Sharks, and Rays, and even these are by no means numerous; while the four new families of the *Sclerodermes*, the *Gymnodontes*, the *Lophobranches*, and the *Acipenserides*, appear almost at the same time in the family of the Ganoïdes, replacing those which become extinct. The lists of fossil fishes, arranged according to geological formations, which will be found in my work, will render these generalities more consistent, while they will serve, at the same time, as direct proofs of them.

Such facts as these loudly proclaim principles which science has not yet discussed, but which paleontological researches bring under the notice of the observer, with a continually increasing force, I refer to the relations which the creation bears to its Creator. Phenomena closely allied in the order of their succession, and yet without sufficient cause in themselves for their appearance; an infinite diversity of species without a common material bond of connection, grouping themselves so as to represent the most admirable progressive development, in which our own species forms one of the links; are not these indisputable proofs of the existence of a superior Intelligence, whose power alone could establish such an order of things? But such is the severity of our method of investigation, that what we feel to be altogether natural cannot be admitted by our reason, until supported by facts, as numerous as they are well established; and it is for this reason that I have delayed till the last moment to express my convictions on this subject. Not that I have shrunk from the discussions which the announcement of such results must necessarily excite, but because I have not wished to provoke them before being able to settle them on a purely scientific foundation, and support them by substantial demonstrations, rather than by a profession of faith. Upwards of fifteen hundred species of fossil fishes with which I have become acquainted, convince me that the species do no pass into one another, but that they appear and disappear unexpectedly, without having any direct relations to their predecessors; for I do not imagine that it can be seriously pretended that the numerous types of the Cycloïdes and of the Ctenoïdes, which are almost all cotemporary with each other, descend from the Placoïdes and the Ganoïdes. It might as well, in truth, be affirmed that the mammifera, and man among them, descend directly from fishes. All these species have a fixed period of appearance and disappearance; their existence is even limited to a determinate time; and yet they present, when viewed as a whole, numerous affinities more or less close; a determinate co-ordination in a given system of organisation, which possesses intimate relations with the mode of existence of each type, and even of each species. There runs, moreover, an invisible thread, at all times, across this immense diversity, and presents to us, as a definite result, a continual progress in this development, of which man is the termination, of which the four classes of vertebrate animals occupy the intermediate position, and the whole of the invertebrate animals are a constant accessory accompaniment. Are not these manifestations of a Mind as powerful as it is fruitful? acts of an intelligence as sublime as it is prescient? marks of a goodness as infinite as it is wise? the most palpable demonstration of the existence of a personal

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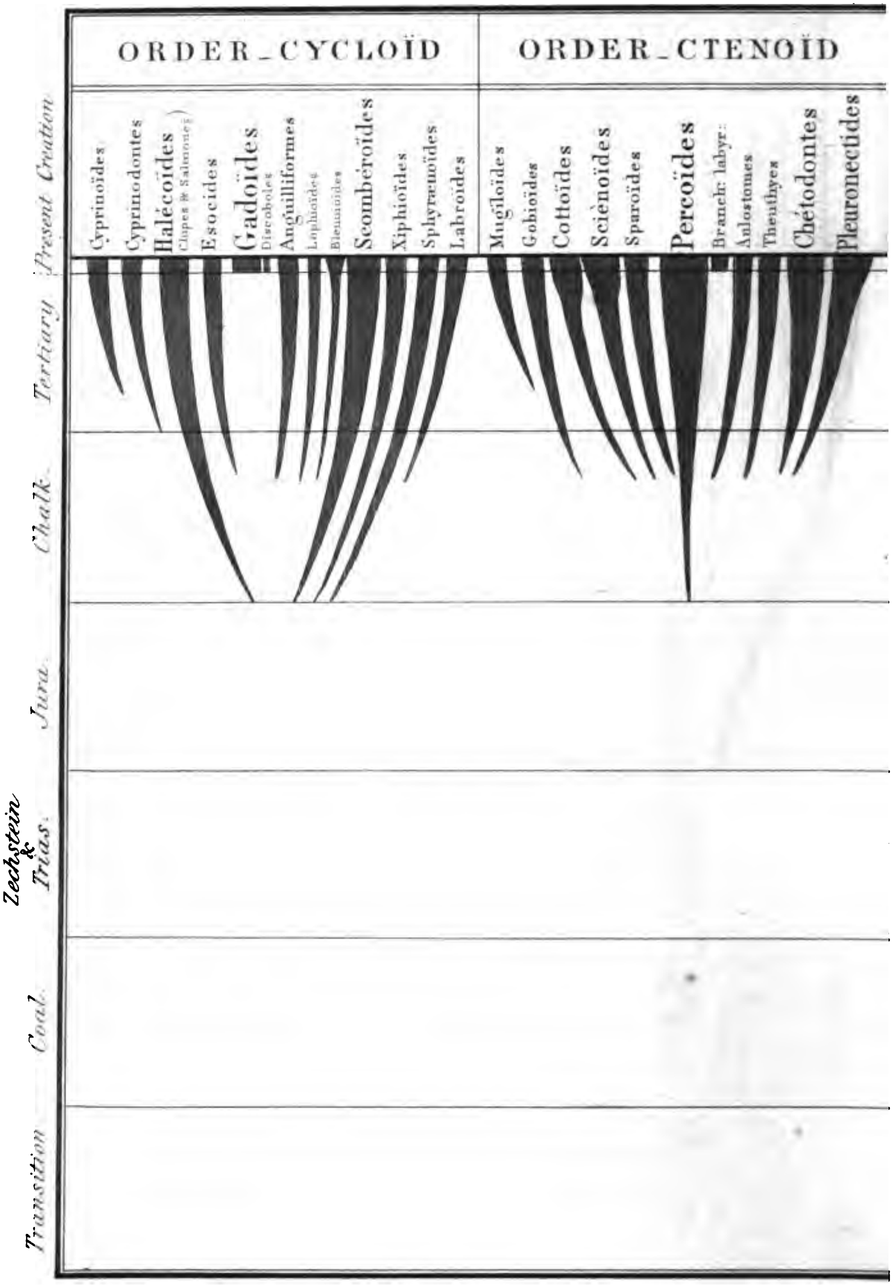

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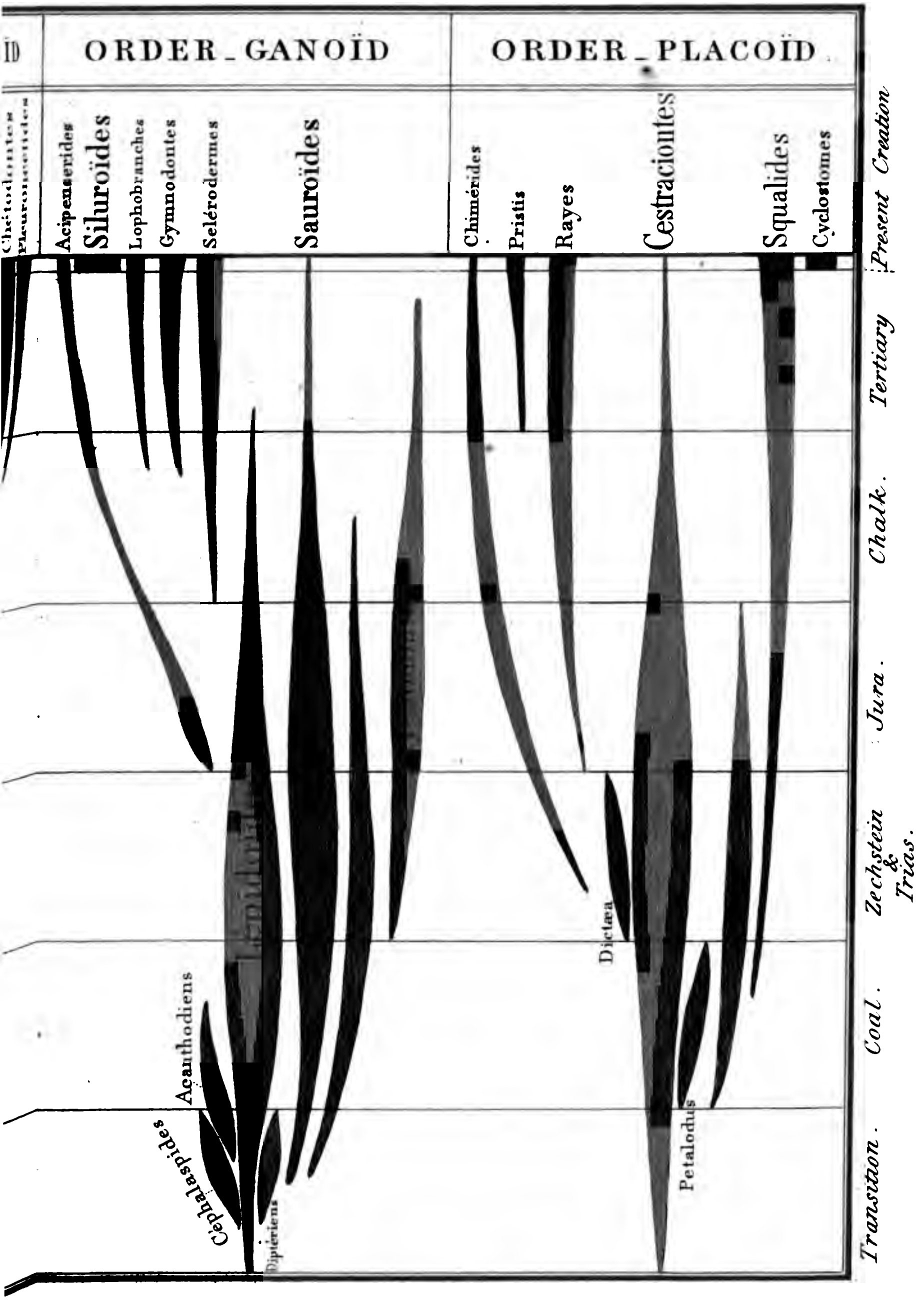

E CLASS FISHES.

Deity, the First Author of all things, the Regulator of the whole world, the Dispenser of all blessings? Such at least is the truth which my feeble intelligence reads in the works of creation, when I contemplate them with a grateful heart. It is a sentiment, moreover, which better disposes us to search for the truth, and seek it for its own sake; and I am convinced that if, in the study of the natural sciences, inquirers less frequently dispensed with touching upon these questions, even in the special domain of direct observation, their progress would generally be more certain and more rapid.*

On the Mode of Formation of Crystalline Limestone, Contact Products, Crystalline Silicide-Slates, and unstratified Crystalline Silicide-Rocks; with Preliminary Observations on the present state of Geology, and on the Methods of Investigation pursued in that science. By B. M. Keilhau, Professor of Geology in the University of Christiania. Communicated by the Author.

(Concluded from vol. xxxvi. p. 362.)

Contact Products.

Besides the crystalline limestones just discussed occurring at the junctions of rocks, there are, as is well known, a number of other mineral substances, which present themselves close to various mountain rocks, in such a manner, as to lead us to conclude that the proximity of the latter has had some direct participation in the production or modification of these substances. If geological knowledge were founded entirely on chemistry; if it is forgotten that the latter science is still so far from having reached that perfection which alone would enable it to solve all the chemico-geological problems; if we are to reject the demand arising out of the very nature of the case, that geological phenomena must first of all be taken into consideration in the establishing as well as in the acquiring of geological knowledge, then, in our opinion, this knowledge would not only be checked in its development, but may be rendered entirely false. This has taken place to the fullest extent with respect to the questions which have arisen as to contact productions. It was firmly believed that here the views of the volcanists could alone satisfy the requirements of chemistry; and as it was a fundamental rule that these should and must be satisfied, so it followed that these products should and must owe their origin to processes put in operation by the great agent of the volcanists. Some phenomena actually produced by heat existed, which, on a superficial consideration, might answer as analogies, and in this way we now find elevated to the rank of a doctrine in our science what should never have been more than a mere provisional opinion. This mode of at once regarding as certain what should have been based on a complete investigation of all the facts, has also here produced its usual effects. No attention has been bestowed on a multitude of facts and special circumstances which are most intimately related to the subject, while in the descriptions other facts have been adapted to that doctrine, nay, under its influence even ornamented with additions, which it would give subsequent observers some trouble to rediscover. That, for example, the changes of the kind we are now discussing, which have been instanced by keen partisans as consisting of actual conversions into melted masses and slags, will in so far be shewn by later observers to be entirely incorrect, is a conviction which I cherish, and which, trusting to the impartial decision of futurity, I am not afraid to express.

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* From Recherches sur les Poissons Fossiles par L'Agassiz. Dernière Livraison. 1843.

Let us suppose that the object of study be those changes that have occurred in certain rocks near a bounding mass, and which consist especially in this, that the rock has become harder, and has acquired a considerable quantity of silica; and that, at the same time, it matters not whether sooner or later, the knowledge of the following facts has been attained:

1. In the Isle of Portland, there rests immediately on a stratum belonging to the oolite group, a bed which contains a quantity of trunks of trees completely converted into a siliceous mass. Round these the rock is also harder and contains more silica, than where it is not in the vicinity of the trees.

2. In the Peninsula of Melazzo, a very new ("quaternary")

limestone reposes on granite and gneiss. As the fundamental rock, during the deposition of the new masses, was full of open fissures and rents, the limestone is also found in these. Thus the limestone is very frequently in contact with the siliceous rocks. It is there to be seen firmly cemented to the subjacent mass, and possessing great hardness; in other words, it has become siliceous.

3. In the *Plauensche Grund*, near Dresden, the *Pläner formation* reposes on a syenite, which is held to be undoubtedly of older origin. At certain points, the former has filled up fissures in the syenite, and in these fissures, at least, the limestone has been found converted into a fine granular mass resembling hornstone.

4. In Auvergne, it has been observed that tertiary strata, which lie on granite (a granite which in many places passes into gneiss), possess a greater degree of hardness near that rock, and that the new masses and the granite are so much intermingled at their common boundary, that it is difficult to distinguish them. (Bull. de la Soc. Geol., t. xiii. p. 220.)

These facts will be treated in a different way by different geologists. He who in his investigations sincerely wishes to obtain a correct result, even though it should be in opposition to this or that system, will first of all, without having the smallest doubt on the subject, recognise all these facts as belonging with perfect justice to the category in question. We may assume that, in the next place, in the problem regarding the cause of these contact phenomena, he will test the hypothesis of the volcanists. If he be but convinced of the accuracy of the data adduced, he must soon become aware how little applicable in these cases is that hypothesis. As to the first example, the masses near which the silicification has taken place, are of such a nature that no one supposes that they have been in a melted state, or have had a high temperature; and in both the two next instances, the altered deposit is in contact with rocks, which, if they were ever in a hot condition, undoubtedly were not so at the time when the new masses were in juxtaposition with them. When the superimposed deposits were formed, these rocks presented an ordinary, weathered, fissured, and undoubtedly very old surface. With regard to the Auvergne granite, it certainly cannot be supposed by any one to belong to the tertiary or to a still newer epoch; and if it ever was hot, this must have been very long before the deposition of the strata which have been modified by it. It will hence be concluded, that such changes as those spoken of can go on at the ordinary temperature. It is next to be considered, whether more general information is to be derived from the same facts. Unfortunately, the result will so far not be a brilliant one; but, as I have repeatedly said, the reasonable investigator does not expect this. Here, again, we have effects of actions which are, for the most part, only exhibited in one or other of such effects produced long ago, and, in trying to account for which, while we are groping but too much in the dark, we are again reduced to call upon electro-chemical currents, molecular displacement, cementation, &c. It may possibly be considered as essential, that the masses by which the silicifications have apparently been effected, are themselves very rich in silica. It is to be remarked, that the inquirer, by having obtained these moderate results, has still reached a point, whence he cannot be so easily led into error by the discussion of other facts connected with the subject. For example, should any one, in order to support the hypothesis of the volcanists, refer to the thousands of places where silicification has taken place in the vicinity of rocks which, according to the prevalent opinion, are pyrogenic, the answer would at once be, that even though this view were correct, still there is no proof that the heat had caused the change; nay, if due weight be given to facts like those adduced, it must, on the contrary, be concluded, that this change near the really pyrogenic rocks has likewise happened in their cold condition.

But let us now return to our examples, in order to see how the supporters of the prevailing school would treat, or have really treated of them. The first of them will probably be dictatorially rejected as inapplicable; or, at all events, it will be exclaimed, this is a mere bagatelle, an isolated fact, which is not to be taken into consideration! As it is always the practice to endeavour to give the method followed an air of strictness, the principle is announced, that conclusions are only to be formed from the totality of the observed facts; but notwithstanding this, all such as do not suit the deductions are excluded with the greatest ease as "accidental anomalies."

With respect to the phenomena presented at Melazzo, some expressions of a distinguished geologist, viz. Constant Prevost, can be adduced. Notwithstanding that it is evident, according to F. Hoffman, whose testimony the volcanists will not reject, that the deposition of limestone has taken place on an old, fissured surface of rock;* yet Prevost finds it difficult to decide "si c'est le calcaire qui a pénétré les roches felspathiques, ou *bien si ce sont celles-ci qui ont passé à travers une vase calcaire.*" The last alternative is adapted for those who wish to see, even here, an effect of volcanic agency; it is also said that the gneiss has evidently been violently moved since the deposition of the limestone. Moreover, an astonishment is expressed regarding this simple phenomenon, which is inexplicable, if the observer does not, at the same time, mean to say—I have found a gneiss, a granite, and a pegmatite, which were pressed up during the quaternary epoch, and which, as is shewn by the alteration of the limestone, were still hot at so late a period! The author writes to Cordier in Paris,—"La presqu'île de Melazzo m'a offert des faits tellement curieux que je n'ose en parler sans avoir des pièces de conviction à faire voir en même temps." When a geologist like Prevost can deceive himself so far; and when it can be supposed that a geologist like Cordier can be convinced that there exist granite, gneiss, &c. which have been solidified either at or since the quaternary period, merely by having placed before him specimens of these rocks, with attached indurated quaternary limestone, we have signs which bear evidence that the science is not in the best possible condition. In the present case, in order to make all surprise vanish, and to render every absurd supposition superfluous,† it is merely necessary to group the fact in a natural way, and to place it along with other analogous cases in such a manner, that true resemblance alone is taken into consideration, and not the arbitrary requirements of particular systems.

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* "There can be no doubt as to the infiltration of the limestone, which has penetrated the gneiss to a depth of ten feet beneath the surface." (*Karsten's Archiv.*, xiii. p. 345.)

† I think it necessary for me to say expressly, that I do not mean to assert that it is impossible that granite *can* have been produced at the quaternary epoch.

An opinion which has been really expressed can also be brought forward respecting the phenomenon near Plauen. The observer who described the appearance says, that the hornstone-like and fine granular nature of the limestone mass, leads to the conclusion, "that a complete chemical penetration of carbonate of lime mixed with siliceous jelly into the fissures of the syenite, has taken place." It is, thus, not here assumed that the peculiar constitution of the *Pläner* formation, where it is in the vicinity of the syenite, belongs to the usual contact-actions; and why? Most assuredly because the syenite cannot be asserted to be newer than the *Pläner*, or, in other words, because it is impossible in this case to assert that the syenite operated by heat. On this account, the above unnatural hypothesis was had recourse to, and thus it is probably assumed that the proper homage due to chemistry has been rendered. If I am not mistaken, we have here an example which, though otherwise of little moment, is yet very instructive, from its exhibiting, in a striking manner, to what the method at present employed often leads; viz. to misapprehension and suppression of important facts, and to the formation of artificial ideas, rather resembling a profitless play of fancy, than the anxious endeavour to discover the truth.

The contact phenomenon in Auvergne also receives no particular attention from the volcanists, as it is here impossible to cite the granite in a hot state as its cause.

I shall here give a few of my own observations, chiefly for the purpose of shewing, that a complete study of the silicifications occurring in certain rocks, requires the consideration of a number of, as it would seem, very complicated relations, which, notwithstanding their importance, enquirers may be very easily induced to overlook, under the guidance of the existing school: 1. Contact indurations often do *not* exist at all in the vicinity of masses which are not less regarded as pyrogenic by the prevailing school, than others near which these changes are met with in a high degree of development. Thus, such indurations are not observable in the Christiania district near those masses of porphyry and greenstone which have more or less the form of beds, notwithstanding that, in other respects, all the circumstances seem to be the same as those in which the silicification of the slate so frequently presents itself. 2. The very extensive silicifications of the clay-slate, occurring in the same district in the vicinity of the great masses of granite and syenite, cease where the boundary between the slate and the granite or syenite passes near the underlying gneiss. There, instead of the usual horn-slate, we regularly find perfectly soft alum-slates, sometimes containing embedded needles of chiastolite. Supposing that the gneiss has there really performed an active part, this may, perhaps, be in some respect compared to the operation of catalytic bodies. 3. Where, in the same district, whole zones of several thousand feet in breadth, round the granite, consist of altered strata, it very often happens that, near the boundary of these zones which is away from the granite, some completely unaltered strata are to be observed between others which are altered in the usual manner. This occurs in the most remarkable manner at a place where the strata do not strike towards the granite, but past it. There we find that, where the altered zone passes into the unaltered slate-formation, there is a regular alternation of modified and unmodified strata, so that *several of the indurated strata are entirely separated from the granite by unindurated strata.*[*] (Gaea Norvegica, i. p. 16.)

The necessity is evident of observing such relations, and not passing them over in silence, whether they lead to this or that result. That hitherto similar phenomena have been little or not at all noticed in other places, plainly arises from the mode in which geology is at present prosecuted. If the favourite theory were placed aside for a time, and if more truly philosophical principles were adopted, a multitude of hitherto unnoticed facts would be discovered, and new light would be thrown on many obscure subjects.

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[*] G. Rose, in his *Reise nach dem Ural*, gives a description and representation of a cliff near Orsk, which, in its upper portion, consists of hypersthene-rock, under which lie jasper and clay-slate. Of the last, it is said that it contains many beds of flinty slate. As Rose regards the jasper as an altered clay-slate, and as the flinty slate can only be a transmuted clay-slate also, and that merely in an inferior degree to the jasper, we have here, undoubtedly, a phenomenon of the same kind as that occurring at the junction of the granite near Christiania.

The information to be derived from contact indurations is very important; and what they tell us of their own origin has also its application to the question of the formation of certain mountain rocks. Clay-slate, where it is in contact with granite, is frequently of the nature of hornstone; but the change likewise proceeds much farther. The hornstone-like condition is but the beginning of a series of modifications, which at last exhibits to us gneiss-formations as a product of conversion; sandstone strata, which, at some parts of their junctions with other mountain rocks, have only become harder and more homogeneous, are, at other points of the same junctions, converted into mica-slate or crystalline quartzite; in short, both these investigations regarding indurations and silicifications have to deal, not only with the production of these themselves, but also with the mode of formation of a whole class of important rocks. And yet, with how little attention geologists have gone to work in the examination of these phenomena! After it had been found that the indurations in some degree resemble imperfectly melted masses, and with the possession of the undeniable fact, that melted masses of great volume must act with a fusing effect on the sandstones, slates, &c. with which they come in contact in the hot state, it was without hesitation assumed as a general principle, that all the indurated slates occurring at junctions are more or less perfect products of this description. Here the irrationality of the method of investigation is very evident. Not only, in deciding as to whether a rock is of eruptive and pyrogenic origin, have the contact-changes occurring near it been employed as an infallible criterion, but, as already hinted, the important question, which must necessarily arise in a cautious and logical investigation, has been entirely neglected or suppressed,—whether, namely, the phenomenon is really exclusively connected with such rocks as may be supposed to have been at one time in a hot state? and, in so far as it occurs near masses which have actually at one time possessed a high temperature, whether it was not produced *after* these were cooled?* As to the chemical part of the question, but little regard has been paid to those strict principles which have been laid down respecting the mutual relation of geology and chemistry; for, in so far as the altered deposits have become siliceous or richer in silica than they previously were, this is a matter in which chemistry is still pretty much in arrear.

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* If we turn our attention to the subject, we may convince ourselves that contact-actions still go on. In the coal-field of Northumberland, hydrogen gas con-

When an uncrystalline slate, like that already noticed, has been converted into gneiss, for example, at a junction with granite, individual crystalline portions of felspar, mica, and quartz, have been formed at the boundary; and these minerals are properly distinguished as actual contact-products, in contradistinction to those results of juxtaposition which consist of more or less considerable modifications of previously existing masses. There is a great number of these contact-products; as well of those which occur as disseminated minerals, as of such (particularly ores) as constitute very extensive masses of different mineral species. Among many other topics connected with these remarkable products, which I cannot now discuss, are the following:—Their subdivision, according to a more exact determination of their situation, in so far as they either occur *in* one or other of the rocks which are in contact, at a little distance from the junction, or actually *between* them; and their subdivision into those in regard to which it may be assumed that the material existed in the immediate vicinity of the places where they are now found, and those in whose case such a supposition appears to be inadmissible. At present, I shall only treat of these products, in so far as is incumbent on me, in order still farther to make good the assertion, that the present practice in geology is essentially deficient, inasmuch as nothing is listened to regarding contact-phenomena, except in connection with volcanism, and then, of course, only in its favour; the actual contact-products are perseveringly and obstinately regarded as the mere result

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tinues to be evolved at the side of a vein of basalt. (*Boué's Jahresbericht* for 1822, p. 12). In the *Magazin for Naturvidenskaberne,* vol. ix. p. 72, attention is directed to a fact which probably bears on the same subject. It is there stated, in a series of observations to determine the intensity of terrestrial magnetism in a portion of Central Europe, that there is an extremely remarkable magnetic relation in the valley of Fassa, precisely where the well-known granite of Predazzo comes in contact with the limestone.

of sublimations from furnaces in the interior of the earth, and of fusions, in short of the action of "fire." On this subject, a multitude of important facts are partly denied, partly misinterpreted, and the path is obstructed to points of view, from which new and instructive considerations might be obtained. I shall not be discouraged by the inattention hitherto paid to my repeated attempts to direct attention to the many indubitable facts, which shew, that the generally received doctrine of the pyrogenic origin of contact-products is hasty, and must be retracted. I continue to beg that geologists may test such facts, of which, therefore, I shall here also adduce a group. In my memoir, entitled "*Einiges gegen den Vulkanismus*" (p. 75–6), I brought forward the following:—

a. Near Commern, there rests, on greywacke, a transition limestone, which, at the junction with the former, contains large masses of ironstone, that are mined.

b. In the Harz, also, beds of ironstone lie at the junction, between limestone and greywacke.

c. The ore at Rammelsberg, near Goslar, is situated between clay-slate and greywacke-slate.

d. At Zellerfeld, in the Harz, the mine of Herzog-August is excavated in a vein, which has limestone on the one side, and clay-slate on the other.

e. Near Iserlohn, the masses of calamine are placed between greywacke and limestone of the coal-formation.

f. The mine Tschakirskoy, in the government Kolyvan (Asiatic Russia), is situated in a repository of ore, at the boundary between limestone and clay-slate. Near Nertschinsk, a similar repository lies in the same manner, between the same rocks.

The following are additional examples:—

g. Near Brzezina, in Bohemia, a red ironstone occurs, which a geologist of the dominant school of geology thinks he cannot regard as of pyrogenic origin, but which is considered by him as a deposit produced by mineral water. Between this substance and the surrounding greywacke there are found—compact cinnabar, heavy spar, iron-flint, and iron-pyrites, which are extracted by mining operations. (Nöggerath's *Ausflug nach Böhmen*, p. 384.)

h. "The great repository of iron-ore in Elba, occurs as an irregular mass, between slate and limestone" (F. Hoffmann, in *Karsten's Archiv.*, vol. xiii. p. 31). It is well known that this slate is a very new one, and that a part of it contains vegetable remains.

i. At Sandomir, in Poland, the numerous masses of ore are generally found at the boundary between transition-limestone and a quartzite-like sandstone, belonging to the same group (a greywacke formation). (Pusch, *Geognostische Beschreibung von Polen.* vol. i. p. 73).

k. Near Kielce, in Poland, there occurs, resting on transition-limestone, a deposit of red sandstone, whose lowest bed is impregnated, for a fathom, with lead-ore. This takes place at several places in the same neighbourhood. As at that locality the lead-ores otherwise belong exclusively to the limestone, or to the transition series in general, Pusch (l. c. p. 76), not without reason, also ascribes the so-to-speak merely parasitic ore in the superimposed newer formation to the limestone.

l. The great bed of Miedzianagora, which contains copper, iron, and manganese ores, and has been mined for centuries, rests on calcareous slate, and is overlaid by quartzite, with strata of slaty clay ("*Letten*") and clay-slate; the dip being from 30° to 40°. The mean thickness of the bed of ore is from 2 to 3 fathoms, and its known length is upwards of 3 English miles. Pusch instances a great number of similar cases in the same district, in which the ore is met with in the same position, close to the junction of the limestone and quartz-rock (l. c. p. 76–91).

m. The beds of hematite, occurring in the southern districts of the State of New York, usually lie near the junction of the talcose slate-formation with a newer limestone. (Silliman's Journal, vol. xl. pp. 75, 76).

n. In New York, masses of iron-glance occur as a contact-formation. The position of these sometimes extremely large repositories of ore "is confined to the upper portion of the primary strata, and the lower layers of the Potsdam sandstone," (l. c. pp. 81 and 82).

o. In Wales, in the midst of Cambrian slates and greywacke, there rises the mural ridge of Cerrig-Mwyn, a mass of grey quartz-rock, which is sometimes brecciated. In contact with this projecting mass, which, in so far as the strike at least is concerned, is parallel to the bounding slates, the latter are much indurated, and contain very considerable portions of lead-glance. This ore is likewise accumulated in large quantities, just at the side of the quartz-rock. (Murchison's Silurian System, vol. i. p. 366).

Regarding all these facts, I must first be allowed to offer some incidental observations.

The whole of the examples now adduced, exhibit masses of ore as contact-formations. Although it appears that it is really metallic minerals which most frequently present themselves in this manner, still, the one-sided tendency of geology must still so far bear the blame, that, up to the present time, it is difficult to find in descriptions, other substances than ores mentioned, as belonging to junctions of rocks whose non-volcanic origin cannot easily be called in question. It is only when one of the rocks in contact is regarded as pyrogenic, that the theorizing geologists trouble themselves with noticing the peculiar mineral products occurring between them; and, on this account, we are acquainted with innumerable examples of such cases, in masses which meet the crystalline siliceous rocks. We are indebted almost exclusively to those occupied with the practical department of the science for a number of instances of a different description; but, from this cause, attention has naturally been chiefly directed to the ores, and not to the other substances belonging to this group of mineral products.

With respect to the example indicated by the letter *d*, it must be remarked, that when the repository there noticed is designated a vein, perhaps no error has been committed. It is, however, certain, that very many contact-repositories have been improperly termed veins; for where one of the two rocks in juxtaposition is unstratified, the contact-masses are, for the most part, quite irregular and disposed in lumps, and altogether do not run so uniformly as true veins, which lie between the sides of a rent or fissure.*

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* I have formerly expressed the opinion, that to refer by far the greater number

Pusch remarks in regard to examples *i* and *l*, that these masses of ore were originally nothing else but strata of one or the other of the including masses of rock, which were metamorphosed to what they now are. That this geologist, who in other respects does not seem to oppose the prevalent geological opinions, should have recourse to such a view, which certainly would only be adopted in the greatest need, must be a proof of the absence at the locality cited of all arguments in favour of the volcanic hypothesis. As to the opinion itself, undoubtedly it can only be approved of in part; we are certainly forced to the avowal, that the spaces which are now filled with ores were formerly occupied only by a mass of barren rock; but the assertion that *this mass* has been *converted* into ore, belongs to those modes of speaking which set all experience at defiance, and which properly deserves blame. If it be the intention by this expression only to prohibit absolutely the natural idea, that the material for the ores was conveyed from without, but by some means not yet explained, then there is here an unseemly anticipation; and if it be only meant that we still know absolutely nothing about the origin of such products, then the selection ought not to have been made of an expression which may so easily lead to misconceptions.

It may easily be imagined what an unyielding volcanist will say to examples *m* and *n*; it is only necessary to recal Prevost's expressions regarding the appearances in the Promontory of Melazzo. I have brought forward these cases, because they afford good examples of the phenomenon observed by me in Norway, and which, upon the whole, seems by no means to be of rare occurrence, in which not merely modifications of the masses in contact have been produced, but even entirely new products have been called forth, where older rocky surfaces are covered by deposits of a newer period. The observer who reports on the above mentioned iron-ore, and who is undoubtedly a perfectly impartial witness, says, "that the ore appears as a bed lying between the primitive rocks

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of true fissure veins to the category of contact-formations, is an idea which would probably be far from unproductive for the theory of veins in general. Compare farther on.

and the oldest of the sandstones;" and he endeavours to shew, that though there are numerous other places where this ore has no other connection than with the primitive rocks, yet at one time the latter must certainly there also have been covered by sandstone. It will be believed that this description is quite fair and free from blame, when it is known that the author, notwithstanding what he has adduced, regards the masses of ore as veins in the primitive rocks.

I will not conceal, in regard to the last example *o*, that Murchison considers the quartz-rock with which the ore is associated, to have been probably influenced by a trap-rock concealed beneath. He who arranges the phenomena of Nature according to a favourite notion, instead of allowing himself to be instructed by what Nature actually exhibits, has here in this manner a path prepared for him.

Let us now proceed to the actual application of these facts. They were introduced, in order still farther to prove, that there is good ground for complaining of the method at present pursued in geological investigations. Although such facts undeniably shew that the general assertion as to the volcanic* origin of contact-formations is unauthorized, yet, hitherto, they have not at all been taken into consideration in the question regarding the production of mineral masses of this description; and, nevertheless, it is, above all, cases like these which are to decide the problem. It is clear that it is not where such masses are, for example, met with in contact with basalt, that we can hope to find a certain explanation, as to whether they are pyrogenic or not, so long as basalt is considered only as a volcanic rock. If such products are attended to only where they occur near rocks of which it is asserted that they have been in a melted condition, it is plain that geologists here follow a course which was least to have been expected from investigators who profess to adopt strict philosophical principles, and who maintain that they pursue a method which infallibly leads to the truth. Doubtless, this course is extremely well adapted for keeping up the system which has been established, for it shuns no arbitrariness which can advance the cause; and reasoning in a circle is quite suited to it. In this way it is impossible to approach nearer to indications of the real truth; on the contrary, by such a mode of proceeding, the path to truth is blocked up.

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* In making use on the present and many similar occasions of this expression, to which many may object, I do not think that I am essentially in error, but, on the contrary, that I am really in the right.

The following is the undisguised and true geognostical result as to the subject of which we are now treating: that where two different rocks have been in contact with each other for a long time, there occur, in innumerable cases, peculiar mineral masses, in whose formation this meeting of the two rocks must have performed a very essential part. As, at least in many of these cases, it is quite certain, that the two touching rocks have always had only an ordinary temperature, that hypothesis (which is, moreover, of little use in explaining the phenomenon) must be rejected, according to which it has been assumed, that the one or the other of the meeting rocks was in a hot condition during the formation of the new masses.

As, however, from the plan adopted, this datum is falsified,—a datum which at once procures for us insight into the subject, and, at all events, ought to be regarded as extremely important as a point of departure for new investigations,—these fruits cannot be attained, and the opportunity of advancing thus afforded, is lost; and in this way, not only geology suffers, but even chemistry, for the love of which such sacrifices are believed to be made. The loss thus caused to geology is really incalculable. Since Nature has it in her power, without fire, and, as it appears, without water, to call forth such products as the contact-formations, is it not madness continually to attend only to the fire and water hypotheses, in treating of the origin of the many problematical mountain rocks and mineral masses. I cannot here leave unnoticed two cases in which what we are taught by the contact-formations must, as it appears to me, be of great use. The first relates to the occurrence in granite, gneiss, &c., of certain *completely* embedded minerals, which contain some of the very rarest metals and earths, and of which these may be especially named the allanite, gadolinite, orthite, thorite, and euxenite. Mr Scheerer of Christiania, who has gained so much credit by the chemical examination of most of these minerals, properly considers their isolated occurrence in extremely rare small portions, in the midst of enormous masses of crystalline silicide rocks, as a very remarkable circumstance. The rare substances which they contain, were not, as is remarked by Mr Scheerer, so distributed as silicium, calcium, potassium, or sodium, for in that case they must have been more frequently met with. If it be now asked, whether it is to be assumed that, in such a case, these materials were not originally present at the places where the minerals containing them now make their appearance?—I maintain, that, in consequence of the phenomena exhibited by contact-formations, the answer may very well be in the affirmative. We still know nothing as to whether they were there produced at a later period " by means of an inexplicable chemical process," or whether we must suppose that they were conveyed thither in some way or other from other places; let, however, the study of such invariably parasitic formations be but once undertaken, so that the relations of some of them may throw light on those of the others, and we may then hope to obtain some insight into this subject likewise.

Another case, in which the phenomena of contact-formations may become explanatory, is that of the origin of veins, and especially of metallic veins. In another publication I have already expressed myself on this subject, in the following terms (*Einiges gegen den Vulkanismus*, p. 78): as shifts almost always occur near metallic and mineral veins, and as they generally bring into contact the transverse terminations of halves of beds not belonging to one another, we easily perceive the analogy between the occurrence of such veins in stratified rocks dislocated by fissures, and those mineral products which we have specially denominated contact-formations. The ideas arising from the study of these last mentioned products, become, however, by continued attentive consideration, still more comprehensive as regards the theory of veins. When two heterogeneous masses touch each other, forces are brought into operation, by means of which chemical products are called forth; but is it not also probable, that in the formation of a fissure, the tearing asunder of one and the same rock disturbs the tranquillity of these forces, and brings them into action? Certainly one disturbance or the other of the previously existing equilibrium follows the discontinuity that has been produced, and, at all events, two masses now exist where there was only one formerly. It will be observed, that I am now pointing out how, in general, metallic and mineral veins may be brought under the same category with contact-formations.

I have already remarked, that it cannot really contribute much to simplify the chemical explanation of the occurrence of mineral products developed at junctions, if we assume that one or other of the rocks in contact has had an extraordinarily high temperature. In relation to the subject, let us consider the following appearance described by Leonhard. Near Auerbach, there is, in the gneiss, a "vein" of granular limestone, which, near the walls, is full of idocrase. Leonhard is of opinion, that, when the limestone, in a hot liquid condition, came in contact with the gneiss, separations and combinations of the elementary constituents of the two rocks took place, and that in this manner the idocrase was formed. If the principle were fully admitted, that liquidity must be supposed when such operations as this take place, then, with reference to the chemical explanation of the phenomenon, a necessary reason would appear for bringing forward the hypothesis of the former liquid condition of the limestone. But, at present, the object of this hypothesis can only be to open up the path for farther explanation. Does it actually accomplish this? I doubt it much. It would be interesting to hear what the chemists themselves would say on the subject. But let us suppose that this phenomenon, chemically considered, is really thus rendered more intelligible, and, at the same time, let us see how this advantage is obtained. It is only by doing violence to all geological probability that we can assert that the limestone has had the origin ascribed to it by the hypothesis; for the eruptions which are known to have taken place have never exhibited any thing of the kind, whereas, on the contrary, we meet with crystalline limestone containing minerals composed of silicates, which, we know with certainty, was never in a melted state (see first part of this paper, vol. xxxvi. p. 357). But, moreover, this hypothesis can only be brought forward at the expense of the eruption doctrine itself. So long as this doctrine endeavours to explain phenomena by the general state of liquidity of the interior of the earth, and, when the question is as to the source of all those masses regarded as eruptive, it refers to the great central reservoir, it is undoubtedly extremely attractive on account of its simplicity and intelligibility. When, however, it is required, that not only the rocks composed of silicates, but also limestones, therefore, in short, the most heterogeneous masses, are to be regarded as having burst forth from the interior of the earth, this theory, as has been remarked by others, no longer possesses the qualities just mentioned; for, instead of producing light, it only leads us into still greater darkness. If, however, it should be the case that the geognostical phenomenon in question is less correctly represented for the purpose of rendering the eruption-hypothesis available, it must be confessed that that doubtful relief is dearly purchased. I do not, indeed, venture to assert that the description quoted of the phenomenon at Auerbach is incorrect, for it scarcely contains any absolute impossibility. It can very well be supposed that the gneiss received from above the upfilling of limestone into an existing fissure, nay, if necessary, we may even imagine a filling proceeding from beneath, and standing in no connection whatever with volcanic action; for, it might be assumed, perhaps, that an internal mass of limestone had been brought to the state of a sort of *moya*, in some way or other, for example, by the movement of the overlying rock and the entrance of water, and that in this way it could be pressed upwards into the fissure. But the real question is, if we have here actually before us a filling up of a fissure, and if the phenomenon has not assumed that character in the description on account of the theory. Leonhard terms the mass a vein, and, from his sketch, the conclusion must be drawn that it really cuts through the gneiss strata; but, in the description which I have seen (*Leonhard's Populäre Vorlesungen über Geologie*, vol. ii. p. 215), this latter and most important circumstance is passed over in silence, which seems not a little suspicious. Is it not the case here, as has undoubtedly happened in other similar instances, that an error has been committed in making a vein of a mass which ought, perhaps, more correctly to be included among the beds?

But it is now time to close these remarks on contact-phenomena, and to pass to another important subject, in the consideration of which erroneous paths have also been followed, in consequence of the prevailing, and, as it is pretended, highly philosophical, but, in fact, altogether incorrect maxims. It is to the crystalline silicide slates that we are now to direct our attention.

CRYSTALLINE SILICIDE SLATES.

Regarding these formations, we see one party directly denying palpable facts, and proposing the most unnatural hypotheses, in order to keep on good terms with chemistry, whose claim to the office of judge in the matter no one has yet examined with attention; while the other party, overpowered by the evidence of the phenomena as displayed in nature, certainly are *near* seeing the truth, but still, in consequence of traditional scruples, stop short of its full perception, and, at the same time, affect a language which sounds like homage paid to the principle,—that geological results must always be chemically comprehensible.

The hypothesis of the Wernerian school of the direct hydrogenic formation of gneiss, mica-slate, &c., meets with no support either from chemistry or geology, and is now scarcely adopted by any one. Some have brought forward the opinion that these formations must be masses derived from the interior of the earth,* which became what they now are from a melted condition; while others suppose that they are sedimentary products, which have been transmuted by volcanic agents.

The idea of the crystallization of such rocks after a previous condition of liquidity, is shewn to be quite absurd even by the consideration of their petrographical constitution; for they are slates. If such are to be regarded as produced by the solidification of masses which have been in a burning liquid state, then that which receives no support from any one observation is assumed to be possible, and thus the very rule to which these geologists so strongly profess to adhere is directly violated by themselves. The notion that a solidification under strong pressure, or great tension, and so forth, might possibly have produced the peculiar texture in these rocks, should least of all have been heard from those theorists of whom we are now aiming, who, according to their own account, follow such strict principles. But it is still worse that this party must deny the very clearest geognostical facts, in order to maintain their opinion. It has now been observed in many places, that strata of rock, which can be recognised by any one, by means of the usual characters, as masses that have been produced in the mechanical way, and deposited in water, present themselves for a longer or shorter portion of their extent as gneiss, mica-slate, or some one of the rocks now under discussion, and thus plainly exhibit a transmutation in these portions, inasmuch as a direct "Neptunian" crystallization can just as little be supposed here as in other cases. As here the geognostical fact itself decides the question in dispute in the most complete manner, inasmuch as the confirmation of these geognostical observations gives it to us as a pure *result of observation*, as a *fact*, that sedimentary, originally uncrystalline masses, have, at certain points, been converted into gneiss, mica-slate, &c., it must be flatly denied by the just mentioned party that such strata exist, which are partly crystalline silicide masses, but in their other portions have retained that condition which betrays the original formation of the whole by means of deposition in water; and, accordingly, this mode of proceeding has not been omitted.

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* Leonhard includes these formations among those which are of *direct* plutonic derivation; and by this he means that they ascended from the depths of the earth in the form in which they now are (? "*als solche,*" that is to say as gneiss, &c.), and that they were the first solidified crust of the red-hot globe. It is not easy to perceive how both these suppositions can be admitted together.

The opponents of the principle of transmutation have a much better field in regard to conversions that have taken place on the great scale. Where we see no unaltered remains of the transmuted masses, the conviction of the change that has occurred does not follow directly from the geognostical phenomena themselves, but can only be founded on the conclusions derived from other evident cases. As in this way the knowledge was obtained that entire large countries, which are almost entirely composed of gneiss and similar rocks, are enormous altered formations of sandstone, clay-slate, &c., this result did not fail to be termed an *hypothesis;* and under this designation it was brought before the judgment-seat of chemistry. As those who adopted the doctrine of transmutation had committed the fault of not distinctly separating what was the incontrovertible result of geognostical observations from those immature explanations with which they deemed it necessary to accompany this result, it thus undeniably acquired an aspect of uncertainty. Thus proclaimed by its adversaries to be a mere idea, and obscured in this manner by its champions, it was rejected by the chemists, who estimated the whole according to the subjoined chemical suppositions. "The most distinguished chemists of our time," says Leonhard, "have, as was to be expected, expressed themselves strongly against the transmutation theory; they characterized it as founded on an insecure basis. Although there are many of the higher problems of geology which chemistry may not be in a position to solve, yet it certainly does not become the former science to hasten beyond the latter; and especially in such bold hypotheses as those in this theory, geologists require recognition on the part of chemistry as a guarantee. Can we blame chemists for keenly finding fault with the adoption of obscure processes without taking into consideration the how and wherefore, without naming the agent which produced these very strange phenomena, without pointing out whence this or that element in the newly produced formations was derived, and without indicating the manner in which the others disappeared?" Now, it is well to notice that, in this reasoning of the influential author, those chemical speculations brought forward by certain geologists, and which have produced misbelief regarding the result as to transmutations, are alone to be understood as included in the expression *insecure basis* of the "conversion theory;" for the *true* basis of our knowledge of the conversions in question lies beyond the proper limits of chemistry. When two equally good geological *hypotheses*, which regard a subject of a chemical nature, stand side by side, it is then quite proper that the decision should be referred to chemistry. But we repeat it again and again, that this is by no means the case here; for, whoever wishes, can *see*, that transmutations of uncrystalline strata into crystalline silicide rocks *have taken place*. The question no longer turns by any means on the possibility or probability of the transmutations, inasmuch as it is now absolutely certain that they have really occurred. Although, in the mean time, they may be chemically inexplicable, yet this can have no influence on the incontrovertibility of the result. How many other facts are there not which still remain chemical mysteries. When, for example, certain *after-crystals* of augite are found, containing a considerable quantity of alkali, whose origin is incomprehensible to chemists, the conviction is not therefore suppressed, that in this case augite has been transmuted. It is only the explanations added to the matter of fact which can here be blamed by the chemist; and, certainly, in some of these occasion has been given for their propounders being cut short by the "how" and "wherefore," or it has been found necessary to advance beyond the limits within which the experimental investigator must remain, and which are often regarded by him as the boundaries of the science itself.*

It will thus be perceived that, in two respects, we consider that party to be in error, who prefer considering the crystalline silicide rocks as erupted masses which have been solidified from an originally burning liquid condition, instead of the regarding them as transmuted, originally uncrystalline, slates, sandstones, &c. 1. Because that party have turned away from nature, which tells them that such transmutations *are actually met with*, and have addressed themselves to an incompetent authority with the unnecessary question, whether such processes are *possible*; and, 2. Because, after receiving a negative answer, they believe themselves *obliged* to reject palpable facts. If we are not wrong in these accusations, nothing else is required to shew the disposition and the judgment with which these theorists proceed. It is not at present necessary to consider other weak points of their geological result.

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* The lamentable part performed by geologists as to the question of the formation of dolomite also naturally occurs to us in speaking of this subject. Instead of, at all events, at first, adhering simply to what personal observation taught them, namely, that dolomite is incontrovertibly a transmuted carbonate of lime, and instead of enriching their science with this result of observation, as with an incontestable fact, they went with their discovery to the chemists, presenting it in the form of a theory. Now, as this theory could receive no approbation from the chemists, the good people remained standing with empty hands, notwithstanding the great discovery which they had really made.

Let us now turn to the other party, viz., to those who recognise the reality of the transmutations. The mistake committed by these geologists, in not, above all things, bringing forward simply and solely the ascertained truth itself, has been already alluded to. Owing to this error, the general reception of this truth into science has been refused; and notwithstanding that care had been taken just to render it agreeable to chemists, by means of attempts at explanation which were added, nothing was obtained from them but an admonitory lecture, of which a repetition has been given above. By adopting heat as the chief agent in the transmutations, it was expected to satisfy the chemists; and I shall here limit my observations to this circumstance, that the conversion-phenomena are in this manner brought under volcanism.

None of the facts relating to this question are *in favour* of the opinion, and many are *against* it, that heat has been in operation in the conversion of various uncrystalline rocks into gneiss, hornblende-slate, &c. In so far as the transmutations have taken place, where the altered strata meet with certain bounding foreign masses, no case has yet been met with, which shews, as a matter of fact, that such masses have produced the change in consequence of their having been in a very hot condition. It is but pure and mere hypothesis when it is asserted that the rocks here spoken of were in a hot liquid state; and even though they had been so, it is far from being a necessary consequence that the change was effected *at that time.* I have previously directed attention to this point; and I have also already stated that, moreover, there are circumstances connected with the contact-changes which really positively prove that heat did not operate in causing these transmutations.

Those gneisses, mica-slates, talc-slates, &c., that are not at all in contact with the unstratified rocks, near which, generally, meeting strata of different slates are seen to be crystalline, seem best adapted for disproving the idea that the change by which they received their crystalline structure was produced by the aid of a very high temperature. In order to shew whence the great heat came, the most arbitrary hypotheses are had recourse to, but even these fail. When whole countries, from the surface to the greatest depths, are composed of gneiss, it is alleged that the original slaty mass, some miles in thickness, has been heated through and through from the internal general reservoir of melted materials. But what as to the many cases where we find the transmuted beds high up in series, which contain *unaltered* strata *under* the altered? The answer is, the altered strata there are no longer in their original situation; either they have separately been elevated to the place which they now occupy, or the whole series of strata has been reversed! It is really remarkable that people will so designedly deceive themselves. The geognostical facts on the subject are to such a degree *speaking*, that although they have hitherto been but very superficially considered, and then only regarded with a prejudiced eye, yet individual geologists have been induced to waver regarding the hypothesis of the action of heat. We now hear something of *electrical* and *other non-thermal actions*, which were in operation during indefinitely long periods, and which, along with the high temperature, have contributed their assistance to produce the changes.—(Lyell's *Elements*, p. 251.)

But why will geologists not place entire confidence in what the natural phenomena teach with such clearness? By stopping half way, it remains equally impracticable, as we have seen, to give an explanation in which chemical experimental observation shall not be anticipated, while, on the other hand, a true apprehension of the facts is prevented, and a check is put to farther advancement.

UNSTRATIFIED CRYSTALLINE SILICIDE ROCKS.

We have still to speak of the unstratified crystalline silicide rocks. The prevailing erroneous ideas regarding the formation of the crystalline slates, of contact-products, of marble, dolomite, &c., have, in fact, been caused by the view entertained respecting the unstratified silicide rocks. It was here that the irrational principles of investigation first came into operation, and here, that method which has so undeservedly come to be regarded as the true philosophical one, especially found its application; the other questions were mere accessaries to that respecting the last mentioned formations.

It is said that we must assume that granite, syenite, porphyry, amygdaloid, &c., &c., have become what they now are from a melted condition, because this opinion alone is satisfactory to the chemists.

Thus is this important matter settled!

But if the case is, that we need not at all *assume* any thing as to the mode of formation of these rocks; that, on the contrary, by the study of their geognostical relations in a reasonable manner, *perfect certainty can be obtained* in regard to this problem, at least up to a point, which in the mean time must be sufficiently satisfactory to geologists; and if it is the case, that chemistry, on its part, is precisely not in a position to deliver a safe *videtur* in this matter—then does this mode of proceeding not betray either a want of knowledge, a deficiency of sound judgment, or a wilful opposition to what is right?

And should it, moreover, be the case, that the really unprejudiced chemist will not be pre-eminently, much less exclusively, satisfied by the hypothesis spoken of, that at least he cannot consider it as chemically more suitable than the result which can be obtained by geological investigations on the subject, then must the accusation made receive still more weight.

He who studies the geognostical relations of the unstratified crystalline silicide rocks, will undoubtedly meet with many obscure appearances, and with many circumstances which seem suited only to envelope the subject in darkness; let him, however, but pursue his investigations with assiduity, with reflection, and with the sole view to discover the truth, and he will find it to be a *certain matter of fact* that, with a few, partly undoubted, partly problematical, exceptions, these rocks also became what they now are, by having been, so to speak, twice formed, first of all, by any of the usual modes by which rocks are almost formed before our eyes; and next, by an alteration which proceeded on the very spot, in consequence of causes which are still unknown, whereby these rocks received their present petrographical character, which just constitutes the difficult part of the subject. It may be disagreeable and disheartening to us, to be obliged to receive into our science an incomprehensible position founded on experience, and which must stand there as such in all its naked-ness; but what is correct in fact cannot be rejected on that account. If this epigenetic mode of formation, as it may be shortly termed, were even less intelligible than it really is, if even also there were no prospect of our understanding it better in future than we do at present, yet we ought to find no reason in this for casting away the result that has been obtained; for, as it is entirely a matter of fact, it must remain, however burdensome it should become in regard to its farther explanation.

It is not my intention to discuss fully here all the facts which shew that the unstratified rocks containing quartz, and composed of crystallized silicides, are transmutations of what were originally sedimentary, or perhaps also partly eruptive masses; but the position of the matter is such, that the most important at least of these facts must here be brought to the recollection of my readers.

1. Granite, syenite, greenstone, porphyry, amygdaloid, &c., are often found connected, by gradual transitions, with stratified rocks, which partly in a direct, partly in an indirect way, present themselves as formations, which were originally deposited by water. The most striking phenomenon of this kind is when the unstratified rock is entirely surrounded by sedimentary strata, of which some with their terminations, and others with their hanging or lying sides, gradually pass into the epigenetic mass. The assertions that have been made regarding these occurrences, for the purpose of supporting the eruption-theory, are quite contrary to nature. Among the most frequent of the transitions where the stratified rock can directly be recognised as of ordinary hydrogenic origin, are those from fossiliferous clay-slate into diorite and other greenstone rocks. Of those from strata which can only be indirectly recognised as hydrogenic, the transitions from gneiss into granite are the most usual. Here, however, the phenomenon, certainly in most cases, is to be understood in this way, that one portion of the original hydrogenic deposit was converted into gneiss, while other neighbouring portions assumed the character of granite, which two rocks, by means of their close mutual relationship, can of course easily run into each other at their common boundaries. In Norway, there are

interesting and not unfrequent transitions from slates partly approaching clay-slate, partly chlorite-slate, and partly quartz-slate into *Helleflint* porphyry (*Porphyre à base de Petrosilex* of the French), hornstone-porphyry, &c. These porphyries are rendered so much the more instructive by their retaining, even at a considerable distance from the characteristic slate, its parallel structure to a certain extent, so that the original stratification can still be clearly distinguished, although the mass, when examined in small pieces, is a perfectly characteristic porphyry. The same slates pass also into gneiss, so that it is easily explained how strata are sometimes encountered, which consist partly of gneiss, partly of such porphyries together, with complete transitions between the two. Gustav Rose noticed similar transitions in the neighbourhood of Schlangenberg (*Reise nach dem Ural*, vol. i. p. 558.)

2. Each particular kind of crystalline silicide rocks is more especially associated with certain uncrystalline stratified rocks, so that a more or less constant genetic relation evidently subsists between two and two kinds of the two great classes: as, for example, between granite or syenite, and clay-slate, between greenstone and greywacke-slate or clay-slate, or newer slates petrographically similar to the clay-slate, between hornstone-porphyry and flinty-slate, between red porphyry or amygdaloid and sandstone formations, &c.

3. It is possible that, in some instances, errors have been committed in the accounts given of the direct occurrence of fossils in the rocks in question, and that, in such examples, the only petrifactions observed were those which had formerly belonged to other older rocks. But, undoubtedly, this has not always been the case when organic remains have been met with in crystalline silicide formations. It is self-evident that such an occurrence must, on the whole, be a rare one; for the conversion of the original rock into an aggregate of crystals, which is frequently very coarse, must, in most instances, have obliterated every trace of the fossils which have previously been more or less perfectly preserved in the mass. However, these occasionally remain, to a certain extent, preserved, and they then clearly prove that the including mass,

like other fossiliferous rocks, was originally sedimentary. The examples given by Murchison (Silurian System, vol. i. chapters 19, 21, &c.) of the occurrence of petrifactions in different traps or similar rocks, are among the newest facts of this kind; and they can so much the less be rejected by geologists of the present day, because the author, as is well known, is a zealous volcanist. His having in this capacity arbitrarily assigned peculiar names to these rocks containing fossils, cannot much stand in the way of our proper apprehension of their real nature, and only shews how little the system, which it is so constantly the endeavour to support, is applicable to the very phenomena which are so distinctly and fully opened up to view. Murchison recognised perfectly distinct traces of encrinites, trilobites, and other silurian organisms, in masses which, according to the petrographical description given, must be sometimes perfectly characteristic syenite, sometimes greenstone, sometimes a kind of felspar porphyry, &c. As these masses, according to the statement of the author, frequently pass into the stratified and mechanically deposited rocks in which, as their original repositories, fossils generally occur, there is no reason for supposing that the organic remains met with in these crystalline rocks, proceed from petrifactions derived from any other quarter. Murchison has also really acknowledged that these animal remains existed from the first in the masses which he found presenting the characters of the above mentioned crystalline silicide rocks. Lastly, it is also worthy of all attention, that he describes these masses as having, for the most part, the nature of beds, and frequently alternating, in this form, with sandstone, slates, &c., but, nevertheless, sometimes constituting one mass with the amorphous portions of the same rock which frequently occur at the same localities. Murchison's descriptions are in the highest degree convincing: it is perfectly evident that, at particular places, many of the silurian strata have been either partially or entirely converted into trap or other similar rocks. Even the volcanic language employed on the same occasion must contribute to strengthen this conviction; for it sounds quite like irony, and the explanation offered is really a caricature of the volcanic theory.*—It will be curious to see to what mode of escape the advocates of the volcanic system will have recourse with respect to the inconvenient discovery that the agate-balls of Oberstein—masses derived from one of the rocks regarded by them as pyrogenic—contain traces of organisms.

4. Many of the relations of form and extent of these rocks shew directly that such masses were originally sedimentary. Comparatively very thin layers spread over an immense area (of which the most striking examples are afforded by Iceland, and perhaps in a still higher degree by Hindostan) *could only* have been deposited by precipitation from water.

If now, in addition to such positive criteria for the epigenetic mode of formation of most of the crystalline granular silicide rocks, we bring forward all of what is more of a negative character which can be adduced against those two hypotheses regarding their origin which have successively prevailed, the unreasonableness of adhering either to the one or the other of those will become still more apparent.† But, as regards the volcanists, such a clinging to the view once adopted must be considered as quite astonishing when we are told, as I have already stated, that this is done in order to retain the secure foundation of chemistry. This can be but a false colour given to the matter. In the first place, chemistry here affords no secure foundation whatever; to-day it proves that minerals composed of silicides can be formed in the moist way, regarding which, but yesterday, it found that they are of pyrogenic origin. In fact, the confessions of the chemists themselves have also been very different.* It will be remembered, that just a short time ago Fuchs, no mean chemical authority, took up the doctrine of Neptunism. In short, it is a pure fiction that chemistry is in possession of an immovable foundation on which our knowledge respecting the origin of the crystalline silicide rocks should rest. In the next place, it may probably come to pass, that when it is decided before a *perfectly impartial tribunal*, whether, in the case before us, *Epigenism* may not have as good claims to the approbation of chemistry as Volcanism, the answer may be in the affirmative. If the discussions regarding the processes which have been designated by the name of cementation, regarding what are termed "*actions lentes*," regarding molecular movements in solid bodies, &c., have not materially assisted us *in understanding* the phenomena on which such discussions turn; still these phenomena, which chiefly lie in the more limited sphere of investigation of chemistry, are sufficient of themselves to shew, that that description of nature's actions which is to be discerned by means of the study of the epigenetic rocks, can also be satisfactorily recognised elsewhere, and is certainly quite normal.†

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* It may be worth remarking, that if the trap strata really were at first "finely levigated volcanic scoriæ passing into sand" (Murchison, i, 75), which fell to the bottom of the sea in which, at the same time, the whole silurian formation was deposited, even this must tell in favour of *Epigenism.* If epigenism is rejected, it cannot be said that such sandy masses became crystalline trap beds, without inventing processes which never took place.

I may also notice, that a German geologist, who, in his own country, observed many greenstones containing fossils, but, slavishly following Murchison, regards them as volcanic tuffs, nevertheless proves, that these *spurious* greenstones, as he terms them, are, petrographically, completely the same as the others.

† One of the most recent pieces of evidence against the pyrogenic origin of certain granite masses and other similar rocks, is the occurrence in them of what are termed *pyrognomic* minerals.

It must undoubtedly be chiefly owing to want of knowledge of the subject, that the question as to the formation of granite and the other rocks more or less similar to it, has been treated in the manner mentioned above. An intimate acquaintance with more than one part of the subject is requisite; but this is, above all, necessary with regard to the mode of occurrence of these rocks, their relations to other rocks, and, in short, all the phenomena and circumstances which can interest geologists. A proper knowledge of this kind, however, *is partly wanting even in those who are considered as the greatest*

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* Upon this subject I have fully expressed my opinion in my little publication, entitled, *Einiges gegen den Vulkanismus*, p. 65–68.

† One of the most interesting of the newer facts connected with this subject is the transmutation of old glass, regarding which Brewster made a communication to the British Association at Glasgow in 1840. The homogeneous mass produced by fusion and subsequent cooling, had, according to him, acquired a heterogeneous and crystalline structure; the metallic particles had separated themselves, and the siliceous particles "had resumed their position as regular crystals, and arranged themselves circularly round the centre of decomposition." The glass

authorities in the science. It will doubtless be thought improper that I should venture on such an assertion, but I cannot keep it back, as I am firmly convinced that it is true, and that it is necessary to make it. It is undeniable that the difficulty is very great of attaining to a perfect knowledge of the relations of the rocks under consideration. It is only at very few points—and even these are for the most part very difficult of access, and are frequently remote—that these rocks are displayed in such a manner as not to be misunderstood. Most observers only see localities which are of such a nature, that, in their case, theories and imagination come to form the chief part of that delineation of the natural phenomena it is the object to produce.* When this takes place with those who make a personal examination of the subject, we may easily form an idea of the situation of the geologists who must derive their information from descriptions.† The deficiency of knowledge can thus be excused; but still it exists. Were this not the case,—did the many geologists who believe that it is not their province, but that of the chemists, to decide on the question as to the

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was excavated among the ruins of the Chapter-house of the Cathedral of St Andrews.

* The following is a most remarkable example of the extent to which this may proceed with even the most experienced observers. In several parts of Sweden, as is well known, horizontal transition strata are found reposing on an ancient surface of rock, which consists of the upright ends of beds of gneiss. Nothing can be more certain and more simple than this; and the relation can be directly and distinctly observed at many of the points where the two systems of strata meet each other. Nevertheless, it lately escaped the observation of one of the most practiced geologists. During his last visit to Sweden, Von Buch was so far from being able to notice it, that, on the contrary, he asserts that "the gneiss is never in contact with these transition strata, but remains everywhere at a distance, and with a distinct margin."—(*Neues Jahrbuch für Mineralogie*, 1842, p. 282.) From this result of his examination of the localities, the author then forms some most singular ideas regarding the internal structure and origin of these mountains.

† The geological authors and teachers belonging to the last class, who unfortunately are not few in number, are, however, just the individuals who feel perfectly assured on the subject. As an illustration, I may instance a German schoolman, who perhaps never saw a mountain, but has felt himself called upon to write a book on "*The Constitution of the Globe.*" He says, that, with the chemical reasons for the formation of granite from the melted state, those also agree, which are derived from positional arrangement; and these, he asserts, are quite decisive; while, according to his opinion, some objections might be made to the

origin of the crystalline silicide rocks, know how perfectly convincing are the geognostical facts which actually exist as an answer to the question; were they fully aware of the many completely conclusive points with regard to the occurrence of these formations,—then immediately it would be universally perceived how natural and reasonable it is to take into consideration, above all things, the geological data, and then the emancipation of geology, which is so extremely requisite, would be accomplished. The misfortune is, that precisely so long as this emancipation has not taken place, geologists are prevented from supplying what has hitherto been neglected. In the mean time, I hope that no one will be prevented from invariably seeking for information founded on fact, or, moreover, from applying what he has discovered in an unfalsified state; and the period must eventually arrive when geology shall rest on an independent basis. Although it must always continue to be felt as a deficiency, that so large a portion of the globe can only be observed indirectly, yet it is nevertheless possible materially to remedy this deficiency, by properly conducted investigations, in the manner already pointed out; and the existence of the possibility gives us sufficient security that this will happen. It will then, I trust, be rendered evident to every one, that the history of what took place in and with the crust of the earth, composed of mountain rocks, is by no means lost to the extent that certain persons of great influence assert;* and some misunderstandings of the most

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chemical arguments. Certainly the blind know more of colours than this worthy man does of the relations on which he rests with so much confidence.

* I must here be permitted to add some remarks suggested by what is said in Berzelius' *Jahresbericht* for 1841, with reference to an article by Studer. "The geologists," it is there asserted, "who suppose that chemistry must be able to explain all geological observations, have entirely forgotten that this explanation must be founded on something more than chemistry." In this there is truth; and it might appear that he who thus expressed himself, must be at one with the views brought forward in the present paper. This, however, is by no means the case. Immediately after this passage, the author states it as his opinion, that if geologists could but give a correct history of the changes which have taken place in the crust of the earth, chemistry, even in its present state, could give exact explanations of most of them. But we cannot expect that this should ever take place, for it is given us to understand that this history is irrecoverably lost. I do not know what the author would require in such a historical delineation of what has

injurious kind, in which, unfortunately, distinguished investigators are involved, will then be removed; and no one, however great may be his name, will allow himself dictatorially to stamp as "hypotheses" or "false explanations," historico-geological *facts* of the first rank.

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occurred in the crust of the globe; the demands might be so great that geologists would not be able to satisfy them; but in this case it is to be remarked, that, after such a *full elucidation* of what had taken place, there would not be much question as to an explanation, for, in this elucidation, the series of chemical events would also form an element. Should, however, only reasonable requirements be made as to a history of this kind, then it is my opinion, as has already been seen, that this can be accomplished, but that chemistry will be found deficient in a greater or less degree, and especially if it will not admit of any other illustrations but those obtained in the usual manner by experiment.

The following, for example, is a fragment of geological history—pure and undisguised history—in regard to which I venture to think, that it is in all its parts perfectly to be relied on. At some period or other after the strata constituting our so-termed primitive gneiss-formation had been brought into their present upright position, a series of strata were deposited at the transitive epoch, which afterwards were variously transmuted in a gradual manner, and at the ordinary temperature. We see most of them now as clay-slates, but other portions exhibit more or less crystalline masses, which occur as well in the slaty form as in the state of unstratified rocks. These were developed, not quite irregularly, according to their different kinds, in the transition district; thus, in the neighbourhood of Christiania, it is quite evident that always only a particular part of the masses of this formation, viz. solely some of the beds which lie next the fundamental rock, have been converted into a peculiar kind of porphyry; in consequence of which this porphyry is quite generally met with in the form of beds, either directly on the fundamental rock, or at least near it, (Gæa Norvegica, i., plate ii., fig. 6, 7, 8).—Now, must not the chemist, before whom this account is placed, with the request that he will give an exact explanation of the act of formation of these beds of porphyry, confess that the problem is beyond his powers? But this he must confess with respect to many much less important cases. Chemistry is not yet in a position to explain the change produced on glass by long lying, observed by Brewster, and mentioned at p 172; a change which, it may be remarked in passing, is undoubtedly analogous to that of the conversion of a more or less homogeneous rock into porphyry, diorite, &c. Let it then be assumed that, although the chemist be forced to make this confession, he is by no means absolutely unwilling to admit as true the above historical report; but that, on the contrary, he is inclined to investigate the subject more closely, in combination with the geologist; then his reasoning will be as follows: Many things are possible in nature, which are not so in laboratories; in the former case, enormous masses are in operation for enormous periods of time; in the instance before us, we have to take into account that enormous apparatus which is produced by the different mutual position of the beds of the two formations meeting each other;

Analysis of Wines from Palestine, Syria, and Asia Minor. By Professor Edward Hitchcock, LL.D., of Amherst College.

It is well known, that in the discussions which have arisen in this country and England on the subject of temperance, much has been said respecting the character of the wines described in the Bible and other ancient writings. By some it was maintained, "that few, if any, of the wines of antiquity were alcoholic;" that the strongest grape wines of the ancients had in them a less quantity of alcohol than our common table-beer;" "that of one hundred and ninety-five kinds of wine used by the Romans in Pliny's time, only one was alcoholic;" "that amongst the Jews in Judea there was a real difficulty, from chemical and natural causes, in the making and preserving any wines except the unfermented;" "that the wines of Palestine were not alcoholic," &c. (Anti-Bacchus.) A vast amount of curious learning was put in requisition in the discussion of this subject. But it has seemed to me that a few analyses of wines from some of the most famous localities of Western Asia, whence the wines of Scripture were obtained, would do much more towards settling the question as to their alcoholic character, than the most ingenious philological criticisms. And I confess I was surprised to find that no such analysis had been made. I wrote, therefore, to my friend, Rev. Henry J. Van Lennep, American missionary at Smyrna, requesting him to send me specimens of the common wines of Palestine, Syria, and Asia Minor. As Mr Van Lennep was a native of Smyrna, I thought he would be better acquainted with the proper localities than a foreigner, and be more sure of obtaining specimens in an unenforced and unadulterated state; while the fact, that he was educated in this country, would make him fully acquainted with the precise object I had in view. I was particular to request him to send no specimen but the pure juice of the grape, to which no ardent spirit had been added. To my request he kindly attended, though with no small trouble. In a letter dated at Smyrna, Sept. 23. 1842, he says: "I have been a great while in fulfilling your commission for specimens of wine from the Levant. I have met with a good deal of difficulty in obtaining specimens from Syria and Palestine, or rather in getting them transported from thence. For what with quarantine regulations, delays of vessels, &c. it is now more than a year, I think, since

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—and, in this manner, at least the possibility of obtaining light and insight in regard to the subject would be afforded.—All this is very different in its nature and consequences from the course followed, of regarding the above historical report as an illusion, because facts belonging to the province of chemistry are therein stated, for whose explanation that science has not yet found the key.

I wrote to some of the missionary brethren at Beyroot and Jerusalem on the subject. I now forward to Boston, to your address, a box containing the following:—One bottle of wine from Mount Lebanon, one year old, and another from the same place six years old; two bottles from Hebron, age unknown; one bottle from Corfu, age unknown; one bottle from Syria, place and age unknown; one bottle from Cyprus, not old; one bottle from Samos, not old; one bottle from Rhodes, one year old; one bottle from Smyrna, new, that is, about a year old. I hope the custom-house officers will not open the box, and shall therefore write the contents on the outside. But with all the precautions I have taken, I should not be surprised should they all, or many of them, reach you soured. Then, instead of your laboratory, they will take their place in your store-room; and whenever you have salad on your table, you will please pour on the vinegar to my health—a sour health, to be sure !"

Fortunately, this anticipation of Mr Van Lennep was not realised, except that one of the bottles from Hebron contained considerable acetic acid, probably because in passing through so many custom-houses, it had been tested till nearly half of it was gone; yet even this, as we shall see, contained no small share of alcohol. All the other bottles, on breaking their seals, were found in a healthy state. And I may add, that in none of them could I discover any carbonic acid, so that probably the process of fermentation had been completed.

The mode of analysis was essentially that of Mr Brande. The specific gravities were determined by ascertaining the weight of a tubeful of the liquor, and comparing it with the same tube full of distilled water, in all cases at a temperature of 60° Fah. The tube which I employed held 736.4 grains of distilled water, and was suspended from one of the arms of Chemin's delicate balances. The weight of the tube and liquid was, indeed, rather too great for a balance of this description, and I do not think I could be sure of the weight nearer than one-tenth of a grain, although with small quantities the one-hundredth of a grain was perceptible. After weighing the tube full of wine, in order to obtain its specific gravity, it was distilled nearly to dryness, from a small retort into a receiver surrounded by snow, and afterwards, to make up for the deficiency, another small portion of the wine was distilled also nearly to dryness. Enough was thus obtained of the distilled liquor to fill the tube, which was then weighed, and the specific gravity thence deduced. In deducing from thence the per centum of alcohol, I used the new tables of Tralles, founded upon the principles of those by Gilpin, and given by Dr Ure in his Dictionary of the Arts, Manufactures, and Mines. These tables assume that water at the temperature of 60° has a specific gravity of 0.9991; and they give the per centum of anhydrous alcohol by measure. Hence they shew a smaller amount of alcohol than those of Gilpin, used by Professors Brande and Beck, whose standard is alcohol of the specific gravity of 0.825. But as Gilpin's tables have been so commonly used, I have added a column of the amount of alcohol by measure, as obtained by those tables in Brande's Chemistry. The tables of Lowitz of St Petersburgh are also preferred by some. He assumes, as his standard, alcohol of the specific gravity .796 at 60° Fah., and gives the per centum by weight. I have given a column deduced from his tables, also, as contained in the Second Supplement to the seventh London edition of Turner's Chemistry, by Professor Gregory. From the specific gravity of the wine before and after distillation, I have deduced the amount of solid matter, and given the per centum by weight. Finally, I have added a column to the per centum by measure of brandy, on the supposition that brandy contains 49.44 per cent. of pure alcohol.

As others like myself, who may desire to analyse fermented liquors, may not be able to procure Gay Lussac's apparatus for that purpose, I will observe that I used two methods of connecting the retort and receiver, which I consider much better than to lute them together. One was, to make the junction by a strong India rubber tube tied firmly to both vessels by a waxed thread. The other, and still better method, was, to find a receiver whose neck would just admit the neck of the retort, with a piece of firm paper wound carefully around it, and slightly pasted to it. By giving the retort a screwing motion, it was easily made to fit into the receiver so firmly, that there was no danger of leakage.

Results of the Analysis of Wines from Palestine, Syria, and the Levant.

LOCALITY.		Specific gravity before Distillation.	Specific gravity after Distillation.	Per cent. of solid matter.	Per cent. of Alcohol by measure by the Tables of Tralles, sp. gr. of standard Alcohol at 60° Fahr., 0.7946.		Per cent. by the Tables of Gilpin, sp. gr. of standard Alcohol, 0.825.	Per cent. by the Tables of Lowitz by weight, sp. gr. of standard Alcohol 0.796.	Per cent. of Brandy, containing 49.44 per cent. of Alcohol.
No. 1. Hebron, soured, age unknown		1.0097	0.9809	2.85	14.3		15.52	14.2	28.90
... 2. Hebron, age unknown,	1st trial,	1.0083	0.9770	3.10	18.1	17.50	19.50	17.1	35.40
... ...	2d ...	1.0086	0.9782	3.01	16.9		18.32	15 9	
... 3. Mount Lebanon, 1 year old, ...	1st ...	1.0121	0.9812	3.05	14.0	14.15	15 19	13.8	28.62
... ...	2d ...		0.9809		14.3		15.40	14.1	
... 4. Mount Lebanon, 6 years old, ...	1st ...	1.0892	0.9852	9.55	10.4	10.95	11.26	11.9	22.03
... ...	2d ...	1.0880	0.9839	9.57	11.5		12.50	12.2	
... 5. Syria (Port wine), place and age unknown,	1st ...	1.0051	0.9808	2.42	14.4	14.70	15.48	14.3	29.57
... ...	2d ...		0.9802		15.0		16.21	14.9	
... 6. Cyprus, not old, . . .	1st ...	1.0220	0.9779	4.31	17.2	17.05	18.63	16.2	35.49
... ...	2d ...	1.0254	0.9782	4.60	16.9		18.31	15.9	
... 7. Rhodes, one year old, .	1st ...	0.9920	0.9772	1.49	17.9	17.75	19.25	16.9	35.90
... ...	2d ...	0.9909	0.9775	1.35	17.6		19.00	16.6	
... 8. Corfu, age unknown, .	1st ...	0.9930	0.9790	1.41	16.1	15.75	17.26	15.6	31.86
... ...	2d ...		0.9798		15.4		16.61	15.2	
... 9. Samos, not old, . . .	1st ...	1.0205	0.9812	3.85	14.0	14.35	15.19	13.9	29.03
... ...	2d ...	1.0226	0.9805	4.11	14.7		15.91	14.6	
... 10. Smyrna, rather new, .	1st ...	1.0162	0.9826	3.31	12.7	13.00	13.78	13.3	26.30
... ...	2d ...		0.9820		13.3		14.33	13.11	

I was surprised to find so much alcohol as the above Table exhibits in No. 1, which would pass for tolerably good vinegar. No. 2, from the same locality, shews us probably how much alcohol it contained before the acetic fermentation commenced. These specimens were from grapes, grown probably not far from the "Valley of Eschol," whence the famous cluster was borne away by the Jewish spies in the time of Moses; for that valley must have been in the south-easterly part of Palestine. No. 2 has the taste of strong Madeira wine. Nos. 3 and 4 are from Mount Lebanon, one of the most famous localities of the wines of Scripture. No. 3 is astringent and somewhat sweet, yet it appears to be fully wrought. No. 4 has a similar taste, but it is quite thick, as its high specific gravity shews; and I strongly suspect that the grape juice was partially boiled down before it was allowed to ferment, as we know was formerly practised, and is still done, on Mount Lebanon, according to Mr Buckingham. It has the appearance of the other wines, after they have been heated to the boiling point in the retort; that is, a redder colour than is natural. No. 5 is perfect Port wine in colour, taste, and the amount of sediment deposited in the bottle. No. 6 is from Cyprus, which is one of the most famous localities of the ancient Greek wines. It is sweet and astringent, but not thick, and has no appearance of having been boiled before fermentation, as Mr Buckingham says is usually done on that island. It will be seen that it is a very strong wine. The age of those wines mentioned in the table are their ages when obtained by Mr Van Lennep. A year more, at least, should be added, except, perhaps, in one or two cases, as having elapsed before they were analysed. No. 7, from Rhodes, is a very clear strong wine, the strongest which I analysed, and slightly astringent, resembling some varieties of Madeira. No. 8, from Corfu, whose age is unknown, considerably resembles it in appearance and taste, and, as the analysis shews, in alcoholic power. No. 9, from Samos, is less clear, more astringent, and less strong. No. 10, from Smyrna, has the colour of Port wine, and is sour, astringent, and unpleasant, tasting strongly of the skin of the grape. The sourness appears to have been derived, chiefly at least, from the grape, and not from fermentation. It was about eighteen months old when analysed; called, however, by M. Van Lennep, *a new wine.* In short, these specimens exhibit a good deal of variety of character, and are, therefore, favourable for the object in view. It will be seen that, in all cases except the first, which I conceived to be of little importance, I performed two analyses of each specimen; and I have given both results, that chemists might judge how much dependence is to be placed upon my researches. In No. 2, the difference in the amount of alcohol, by the two processes, amounts to 1.2 per cent. In the other cases the difference is less; and it seems to me we are warranted in concluding, that my mean results do not vary more than 1 per cent. from the truth in any case; and this is near enough for all the purposes for which the analysis was undertaken.

It appears that in all cases, except Nos. 7 and 8, the specific gravity of the wines before distillation was greater than that of water. No. 4, from Lebanon, was much heavier; in part, probably, because the juice was concentrated before fermentation, and in part because it is so old. It yields, of course, a large per cent. of solid matter.

The difference in the results, according to the tables used, is just what we might expect from the different standards assumed by Tralles, Gilpin, and Lowitz, and from the fact that the table of the latter gives the per cent. by weight, whereas all the others give it by measure. Gilpin's tables have been most commonly made the standard, but they convey erroneous conclusions; that is, as the subject is usually understood, they indicate more alcohol in fermented liquors than they contain.

The results which I have now given justify, it seems to me, the following conclusions:—

In the first place, the grapes of Palestine, Syria, and the Levant generally, produce wines as strongly alcoholic as those of any country whose soil and climate are congenial to the vine.

It has been thought that the great quantity of sugar which must exist in the grapes of those countries, and the heat of the climate, are so unfavourable to fermentation that little or no alcohol can be produced from them. But here we have ten specimens of the common wines of those countries, all of which belong to the class of strong wines. It may be thought that the strongest wines were selected by Mr Van Lennep; but I particularly requested him not to do it, desiring him to send me rather the common wines; and the apprehension which he expressed that they would be all soured before reaching this country, shews that he supposed them to be quite weak. I incline to believe that their strength is not above the average in those countries; and yet, by consulting the analyses of Brande, Beck, Fontenelle, &c., we shall see that they rank among the stronger wines. And, indeed, this is just what the chemist would expect; for if those countries furnish the finest grapes, they doubtless contain a large amount of the sugar and ferment requisite for the production of alcohol.*

In the second place, we have every reason to believe that the ancient wines of the countries under consideration possessed essentially the same character as the modern wines made there.

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* Since the above was written, I have had the pleasure of meeting Mr Van Lennep in this country, and he confirms all the statements made in the text respecting the strength of the wines. He is even of opinion that those from the neighbourhood of Smyrna are below the average strength of the wines of that region. Rev. Mr Sherman, also, who obtained the specimens from the vicinity of Hebron, and whom I have lately seen, thinks that they may be somewhat stronger than the average of wines in that region. The specimens from Mount Lebanon were procured by the Rev. Leander Thomson, who is also in this country, but I have not met with him.

There has been no important change in the climate, and of course the grapes now produced there, are the same essentially as in ancient times. If the wines are different, then, it must be the result of different modes of making them; and I am not aware of any important difference in this respect, unless it be in those cases (and whether there be any such cases I know not) in which the wines are enforced by the addition of distilled liquor; but such a case affects not my present argument, because I have analysed only those which are derived from the pure juice of the grape. Much, indeed, has been said about the practice of the ancients, of boiling down the juice of the grape, more or less, before allowing it to ferment. But the same practice exists now; nor is there any reason to believe that it was ever general, but resorted to only to furnish an agreeable variety. And it so happens, fortunately, that one of the specimens analysed—viz., from Mount Lebanon—is a wine thus prepared; and it may stand as a representative of that class of wines. It is, indeed, the weakest wine of the number; and we learn from this fact, that this process does affect the amount of alcohol; and yet this specimen contains about 11 per cent. of pure alcohol, and 22 per cent. of brandy,—enough, certainly, to make the wine quite intoxicating. Yet it is quite sweet, and therefore sweetness does not prove that a wine is unintoxicating. When the juice of the grape is boiled down, so as to become thick like honey, or even solid, then, indeed, it cannot ferment, and may be kept an indefinite length of time without containing alcohol. Such was sometimes the case among the ancients; but whether the wine which they called *defrutum*, in which the juice was boiled away only one-half, was of this character, that is, thick enough to prevent all fermentation, I much doubt. This inspissated juice of the grape was rather regarded as honey, and so it is called in the Bible, and at the present day, in the Eastern world, it is a very common article; but so far as I can learn, by inquiring of several missionaries, it is not called wine, but is rather a substitute for our honey or molasses. Admitting, however, that this article was sometimes called wine by the ancients (and I have no doubt of the fact), its use as a beverage must necessarily have been quite limited, and therefore this fact does not invalidate my general conclusion, that the character of the ancient and modern wines in Eastern countries was essentially the same. This conclusion, at which Professor Beck arrived by chemical considerations, in his valuable paper on the analysis of wines in this Journal (vol. xxviii.), seems now to be still farther confirmed by experiment.

I trust that, in arriving at such conclusions, it will not be imagined that I wish to take away any support—or do, in fact, take away any support—from the noble cause of temperance, which I have endeavoured for so many years to sustain, both theoretically and practically. True, some able friends of the cause have supposed the ancient wines to be mostly intoxicating. But I rest, and always have rested, its support on very different grounds than the per cent. of alcohol in the wines of Syria and Palestine. But this is a point irrelevant to the present paper, and therefore I waive it. To find out the exact truth should be the object of every scientific investigation, however it may affect opposing opinions.—*American Journal of Science and Arts*, vol. xlvi. No. II., p. 249.

Description of an Improved Apparatus for Levelling Small Theodolites. By Mr John Sang, Land-Surveyor, Kirkcaldy. With a Plate. Communicated by the Royal Scottish Society of Arts.* With a Plate.

A theodolite of the common construction is levelled by a series of alternate adjustments of two pairs of screws, each adjustment requiring both hands of the operator; its plate is prevented from moving in *azimuth* by the same screws, which have to be gradually tightened at each step of the process, until, when the adjustment is completed, the screws have also obtained the tension proper to keep the instrument steady. The operation requires some address, and occupies a considerable time. By the improvement now described, only one hand is required to regulate each level, and the instrument is kept steady without tightening the screws, so that both levels can be adjusted at the same time, and more rapidly. It gives the same facility as the apparatus of three screws, sometimes applied to large instruments, whose weight is enough to resist shaking in azimuth.

A (see P. II.) is a part made fast to the legs of the instrument. It has two sockets at B B, and a box at C for holding a cylinder containing a screw-nut. The box C is not well seen in the drawing; it is similar to the other one marked G.

The part D has an axis E, working in the sockets B B, each end of the axis is a double cone, carefully fitted into the sockets, and tempered by the screws *b b*. It has, at right angles to this axis, two sockets F F similar to those in the lower part, and it has also a box at G, holding a cylinder containing a screw-nut.

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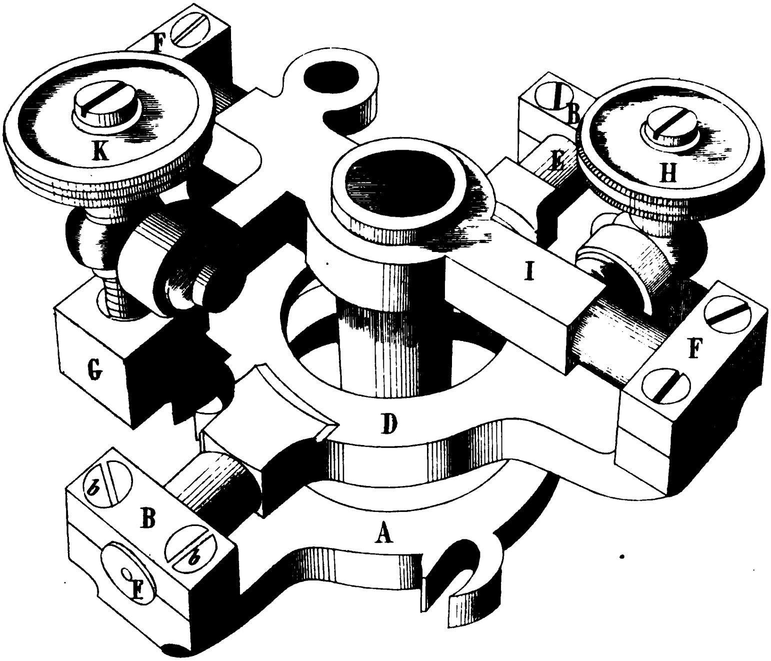
* Read before the Society, 8th January 1844.

Edin. New Phil. Journal Vol. XXXVII Plate II Page 182.

Mr Sang's apparatus for levelling small Theodolites.



John Sang del. S. Leith's Lithogy. Edinr.

The screw H is attached to the part D by means of a revolving joint, and it works in the nut at C, so that, on being turned, it alters the inclination of the part D to the fixed part A.

The part I, which contains the outer axis of the theodolite, has an axis (like E) working in the sockets F F.

The screw K, working in the nut at G, and being attached to the part I by a revolving joint, alters the inclination of I with regard to D; so that, by means of the two screws H and K, the part I can be inclined in any manner to the fixed part A, while there is in every portion of the apparatus a firm resistance to a motion in azimuth.

The small screw L is intended to temper the pressure of a piece of tin, inserted into the nut to make up for the wearing of the levelling screw.

This apparatus will add to the expense of a theodolite, but by no means in proportion to the time which it will save.

JOHN SANG.

KIRKCALDY, *4th Nov.* 1843.

Observations on the Motion of Earthquakes transmitted under the Andes. By RICHARD SOLLY, Esq. Communicated by the Author.

Sir,—Having been lately engaged on a paper for the Sheffield Literary and Philosophical Society, the subject of which was earthquakes, I have been much interested in the valuable communications published in your Journal, especially those of Professor Bischof, Dr Daubeny and Mr Milne. One or two remarks have occurred to me respecting a fact which I have not seen alluded to by any of our geologists, but which you may perhaps consider as not altogether unworthy of their attention. I refer to the shocks of earthquakes having extended beyond vast mountain ranges, and yet having been felt either very slightly, or not at all, in those mountains.

Mrs Maria Graham, in her account of the great earthquake of the 19th November 1822,* which destroyed so many buildings in the town of Valparaiso, and permanently raised the adjoining coast from two to three feet, states that it was felt eastward, beyond the Andes, at Mendoza and St Juan. I heard the same account when I was in Chili in 1828, and have no doubt whatever of the fact. I crossed the Andes that year by the Aconcagua and Uspallata Pass, six years therefore after the great earthquake, and my astonishment was excited by the position of an immense mass of rock called the "Penon rajado," or "Piedra partida," about half way between the Cumbre and the Plain of Uspallata, and 7300 feet above the level of the sea. It had evidently fallen, at some former period, from the cliffs above, and had split into two large pieces and several smaller. The equilibrium of both of the principal fragments appeared so precarious that in spite of the burning sun, one of my companions refused to repose under their shade, being convinced, as he said, "that the slightest shock would overthrow the enormous mass and crush us to atoms."* This seemed likely enough, but nevertheless our head muleteer, Pedro Aransivia, assured us that they had not changed their position in his time, nor in that of his father before him, and that they had always availed themselves of their shelter; also, that there were traditions, respecting the ancient Incas, connected with these rocks.

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* Trans. Geol. Soc. 2d series, vol. i. part ii. p. 413.

Of the great Conception earthquake of the 20th February 1835, which permanently raised the coast two feet and upwards, Captain Fitzroy says, "This earthquake was felt at all places between Juan Fernandez and Mendoza. At Mendoza the motion was evenly gentle. Towns and houses which lay between the parallels of 35 and 38, suffered extremely, nearly all were ruined, but northward and southward of those latitudes slight injury was done to any building."† This account I know to be correct, with the exception of the first sentence, if taken literally, as the shock was *not* felt *in* the Andes, although it was felt *beyond* them, at Mendoza. An intimate friend of mine happened to be in the Cordillera at the time, and I can state positively that neither he nor any of his party knew that there had been an earthquake until their arrival at Mendoza.

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* Extract from my journal, written at the time.

† Narrative of the surveying voyages of the Beagle, vol. ii. p. 418.

These facts are quite in harmony with the theory adopted by Professor Bischof, by Mr Darwin and other eminent geologists, at least partially, which Professor Phillips favours,* and which Mr Lyell requires,† namely, that the nucleus of the earth is an intensely heated liquid or fluid mass. This granted, motion is supposed to be transmitted by subterraneous undulations to enormous distances, proportionate to the strength of the explosion, and the depth beneath the earth's surface at which it takes place. Now, we can well imagine that an immense mass of mountains, such as the Chilian Andes,‡ might ride unshaken (while lower ground suffered) like a ship of the line which moves not with the ripple tossing the little boat at its side.

Nevertheless we know that the Andes do sometimes suffer extreme dislocations; for instance, by the Tacna shock of the 18th September 1833, of which Mr Matthie Hamilton gives a circumstantial account in your Journal.§ He states that after the calamity, when the atmosphere became clear, the Andes, as seen from Tacna, presented a novel spectacle, and in many parts appeared with a new surface, large portions had been thrown off, or had slid into valleys below, leaving some of the more elevated peaks denuded of what had been their more prominent limbs; and he gives many other similar particulars. Indeed the examples of shattered mountains are too numerous to require citing.

Mr Darwin expresses his surprise at the effect produced by the Conception earthquake of 1835, on the island of Quiriquina. He says, " The effect of the vibration on the hard primary slate which composes the foundation of the island was still more curious, the superficial parts of some narrow ridges were as completely shivered as if they had been blasted by gunpowder."‖

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* Treatise on Geology, vol. ii. p. 209.

† Elements of Geology, p. 267.

‡ From 11,000 to 23,000 feet high and from 100 to 200 miles across.

§ Jameson's Philosophical Journal, vol. xxx. p. 153.

‖ Journal of Researches, p. 370.

May we not then assume as correct, both explanations of the mode in which earthquakes are supposed to transmit their motions? by the undulations of the subterranean fluids, and by the vibrations of the superficial crust. Mr Milne has pointed out that most of the English and Scotch earthquakes, being confined to mere patches of the earth's surface, must be due to the latter; and, on the other hand, Professor Phillips remarks that rocks being very imperfectly elastic, owing to the numerous divisions which intersect them, cannot be supposed capable of transmitting vibrations to any very considerable distance. To the subterraneous undulation we may attribute the motion; which passing under, without shaking the Andes, was felt "evenly gentle" at Mendoza. To the superficial vibration we may attribute the fall of the mountain peaks at Tacna, the shivered rocks at Quiriquina, and those strange rotatory motions referred to by Mr Darwin,* and of which I have seen striking examples at Tacna and at Lima. In the latter case the upper stone of a lofty obelisk was turned, in 1828, in a manner precisely similar to that in Calabria, of which Mr Lyell has given a drawing.†

The question occurred to me some time since, though I should scarcely have ventured to mention it without having seen Professor Keilhau's remarks in your last number, whether frequent vibration, repeated during long series of ages (which, according to Mr Darwin,‡ I will suppose to accompany the elevation of a mountain chain), may not have produced considerable changes, all tending to crystalline forms, in the molecular constitution of some of the basaltic and metamorphic rocks. It is now an ascertained fact, though but recently acknowledged, that malleable iron, and other metals in a fibrous state, assume the crystalline, under the operation of vibration, and without the accession of heat.§ My friend Mr William Lucas, (this year the President of our Sheffield Society) has been making some interesting experiments on the changes in metals, and I have requested him to extend them to rocks, which I hope he will do.

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* Journal of Researches, p. 376.

† Principles of Geology, 6th edit., vol. ii. p. 336.

‡ Journal of Researches, p. 380; and Geological Observations on Volcanic Islands, pp. 95 and 129.

§ The axle-trees of railway carriages, and the holding down pins in iron works, afford familiar instances.

RICHARD SOLLY.

SANDON PLACE, SHEFFIELD,
28th May 1844.

On Parietin, a Yellow Colouring Matter, and on the Inorganic Food of Lichens. By ROBERT D. THOMSON, M.D. Communicated by the Author.*

The objects of the present paper are, 1*st*, To endeavour to prove that, contrary to the usually received opinion, the class of plants termed Lichens, require inorganic matter as part of their food, which they must derive from the localities upon which they are fixed; and, 2*d*, To describe the yellow colouring matter obtained from the yellow wall lichen, and to detail its properties, composition, and application, as a test for alkalies.

Although chemists are acquainted with several yellow colouring matters, few of them have been separated in a pure state, and analysed. This arises from the difficulty of procuring such substances in the same state as that in which they existed in the plant from which they are extracted—depending principally on the facility with which they unite with oxygen, and on their consequent conversion into a body of inferior beauty, and of an uncrystallized structure. The yellow colouring matters which have hitherto been analysed, are derived from various parts of phenogamous plants, principally the roots and flowers. The subject of the present paper is procured from a totally different tribe—the lichens—but one to which we are indebted for some important dyes. The Greeks gave the name λειχην to a disease of the skin, and likewise to certain plants possessing the power of healing these cutaneous eruptions. Dioscorides † tells us that the lichen, which is familiarly known from its growing on stones, and attaching itself to the rough parts of rocks, like a moss, was called by some persons *bryon*, and was useful in the cure of sanguineous fluxes and inflammations. Pliny likewise uses the term lichen; but from his describing it as growing on rocks, with one leaf from a broad root, and with one small stem, it is obvious he refers to a species of the hepaticæ.* Galen likewise enumerates lichens among the instruments of cure, in the treatment of impetiginous or cutaneous diseases. Modern botanists, up to a comparatively recent period, appear to have overlooked this class of plants, if we may draw this conclusion from the catalogue of English plants, by John Ray, the second edition of which was published in 1677. In this work, the celebrated author describes, under the title of lichen, eight species of plants, only three of which, however, can be reckoned true lichens, the remainder being hepaticæ and algæ. In Hooker's Flora, published in 1833, there are enumerated and described thirty genera and 420 species of lichens. It is well known that many of these are capable of supplying powerful dyes.

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* From the Transactions of the Glasgow Philosophical Society.

† Mat. Med. b. iv. cap. 48.

The lichen from which the colouring matter to be described is derived, is of very frequent occurrence on walls and trees. It is the *Parmelia parietina*, (yellow wall parmelia), described by Hooker as possessing a rounded bright yellow frond, with lobes radiating, marginal, appressed, rounded, crenate, crisped, and granulated in the centre. The *repositories*, or apothecia, are deep orange, concave, with an entire border. The bright yellow colour of the lichen is a sufficient indication of the presence of a colouring matter; but the real intensity of the colour could scarcely be anticipated merely by an inspection of the plant.

FOOD OF LICHENS.

The most luxuriant samples of the parmelia grow in the neighbourhood of the sea, from what cause, unless it be the moistness of the air, it is not easy to determine. Botanists consider that this race of plants derive no nourishment from the rocks upon which they grow, although the circumstance of many of them containing oxalate of lime would appear to afford a demonstration of their being enabled to suck up inorganic substances in the same manner as other plants. Viewed in this light, the moistening and decomposing effect of a humid atmosphere on the rocks on the sea-coast, may explain the almost herbaceous appearance of some of the lichens which may be observed in such situations. The subject, however, of the nutrition of lichens, is in its infancy, and will require a searching investigation.

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* Nat. Hist. xxvii. c. iv.

It has been already stated that, according to the opinion of botanists (Hooker's English Flora), lichens derive no nourishment from the rocks, stones, or trees, on which they grow. The roots or fibres with which they are often supplied, it is conceived, are only useful in fixing the plant to its place of growth, its nutriment being derived from the air. One of the most common of our lichens, the *Peltidea canina*, possesses fibres on its under surface so closely resembling those of shrubs, that one would be inclined to attribute to them similar functions. The circumstance, as stated in chemical works, of the absence of any considerable quantity of inorganic matter in the composition of lichens, would appear to lend countenance to the view, that gases constitute the only food of lichens. But the fact of oxalate of lime having been obtained from many lichens, seemed to call in question the validity of the conclusion. The detection, also, of small portions of bitartrate of potash and phosphate of lime in some lichens, added still further evidence against the opinion of botanists. So far as I am aware, no other substance of an inorganic nature has been hitherto detected in lichens, except in such minute proportion that it might have been derived, perhaps, from extraneous sources. I was not, therefore, prepared to expect the remarkable results which the analysis of the yellow parmelia afforded. In one experiment, 50 grains, obtained from mica-slate rocks at Dunoon, on the west coast of Scotland, when ignited, yielded 3.4 grains of inorganic matter; and in another experiment, 40 grains, to which, as in the preceding trial, no earthy matter was attached, afforded, by burning, a residue of 2.7 grains. In a third experiment, 7 grains of the carefully selected upper parts of fronds, which had never been in contact with rock, and therefore were free from the suspicion of having extraneous particles mixed with them, after washing, as in the previous trials, yielded, by incineration, 0.47 grains of a skeleton, answering to the form of the lichen, and consisting of silica and phosphates, &c. These three experiments, therefore, give a per-centage respectively of ashes, amounting to 6.8, 6.75, and 6.71.* In all these trials, the colouring matter was volatilized before the lichen caught fire. Another specimen, very carefully washed, and consisting of the upper parts of fronds, yielded 5 per cent. of ash, in which phosphate of alumina formed a prominent ingredient. In proof of the fact that the ash is in no degree connected with the rock, a specimen of *Parmelia omphalodes*, taken from the stem of an ash tree, ten feet from the ground, was ignited, and found to yield 7 per cent. of ash, consisting of silica, phosphates of lime, iron, and alumina. The *Cladonia pixidata*, taken from a wall, and free from all extraneous substances, yielded 6 per cent. of ashes, consisting of similar ingredients. Hence it would appear, that this species of plants contain no inconsiderable amount of substances calculated to serve as vegetable manure. The ash possessed the form of the lichen, and a slight iron tint; it effervesced slightly on the addition of an acid. In one instance, some carbonate of lime was present. On digesting the ash in water, a minute portion was dissolved. This solution, on the addition of chloride of barium, gave a white precipitate, part of which was insoluble in nitric acid. On throwing the sulphate of barytes on a filter, and adding caustic ammonia to the filtered liquid, a flocky precipitate—phosphate of barytes—fell. The addition of an alcoholic solution of bichloride of platinum gave no indication of the presence of potash. Nitrate of silver gave a flocky precipitate, insoluble in nitric acid. The soluble salts, therefore, appear to be sulphate and phosphate of soda and common salt. The portion of the residue insoluble in water, became nearly white when boiled with dilute muriatic acid, and left a gritty powder, which, affording a nearly colourless glass with carbonate of soda before the blowpipe, was obviously silica, with slight impurity. The muriatic acid solution gave a copious reddish precipitate, with caustic ammonia. This precipitate was partly soluble in caustic soda, and consisted of phosphates of iron, alumina, and lime. The latter precipitates being tested with lead, yielded a precipitate of phosphate of lead, soluble in nitric acid. The results of the analysis of two specimens of ashes were as follows:—

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* These determinations were made in conjunction with Mr James Murdoch.

	I.	II.
Silica,	68.46	64.62
Soluble salts, sulphate, phosphate, and muriate of soda,	0.75	—
Peroxide of iron, and phosphates of iron and lime,	22.04	34.55
Phosphate of alumina,	—	0.83
Carbonate of lime,	8.75	—
	100.	100.0 0

From these facts it is evident that this lichen requires the same inorganic constituents for food as other plants, with this difference, that the amount of inorganic substances present in its composition is greater than in higher orders of plants, but in a proportion tending towards that existing in the sea-weeds; another character, therefore, in addition to the general external features, indicating an alliance between the algæ and lichens.

To ascertain if the great abundance of inorganic matter was peculiar to this species, the *Parmelia omphalodes* was incinerated, the specimen being taken from a portion collected by a Highlander on the borders of Loch Venachar, where it is extensely used, as well as generally in the Highlands, with an alum mordant, to impart a fine purple to woollen cloths. Its habitat had been a rock, and portions were selected free from any appearance of suspended earthy particles among their roots; 200 grains gave a residue of 7.8 grains, consisting of substances similar to those already enumerated in the analysis of the yellow parmelia. Part of these, however, may have been foreign. When we compare the amount of these inorganic constituents with those found in trees, the balance appears in favour of the lichens, as shewn by the analyses of the ashes of genuine specimens of lima, sapan, and logwoods. The results are in 1000 parts:—

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* Mr David Murdoch assisted me in the first analysis, and Mr James Murdoch in the second.

	Lima Wood.	Suspan Wood.	Logwood.
Organic matter, . . .	971.255	997.083	971.400
Silica and sand, . . .	1.800	—	7.800
Common salt, ...	—	0.517	0.129
Alkaline phosphates and sulphate,	2.000	0.850	1.371
Phosphate of lime, . . .	0.725	—	1.021
Carbonate of lime, ...	24.140	11.650	18.279
	1000.	1000.	1000.000*

Both of these classes of plants alluded to, however, appear but insignificantly supplied with inorganic matter, when contrasted with some of the gigantic sea-weeds from Cape Horn. 490 grains of one of these enormous inhabitants of the deep supplied me by Dr Joseph Hooker, yielded, by incineration, 116.7 of ashes, equivalent to a per-centage of 23.8.

The introduction of inorganic matter into the substance of trees and lichens, can only be effected by the inferior extremity and surface of those portions which are in contact with the source of this peculiar pabulum of vegetable life; while it would appear that the connexion which we always find to exist between sea-weeds and some fixed rocky position, even in the case of these immense inhabitants of the southern seas, according to some physiologists, only serves the purpose of retaining them stationary in one locality, their food being derived from the fluid in which they are immersed. But whether this be true or not, it is certain that the waters of the ocean are capable of affording nearly, if not all, the inorganic ingredients with which these plants are supplied. Trees and lichens have no such atmosphere, rich in salts, from which they can derive their food. They must be indebted for the inorganic matter which they contain to the soil upon which they grow. Hence, since lichens do certainly contain inorganic matter of various kinds, as appears by the facts detailed in this paper, the inevitable conclusion is forced upon us, that these species of plants are not only nourished by the atmosphere, to which botanists have hitherto appeared to restrict their sources of food, but that they are also capable of extracting inorganic matter from the rocks and trees over whose surfaces they are so largely distributed as humble tenants.

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* In these analyses I was assisted by Mr John Aitken.

PREPARATION OF PARIETIN.

When the yellow *Parmelia* is digested in cold alcohol, of .840, a yellow liquid is obtained, obviously from the solution of the yellow colouring matter of the lichen. When boiled gently the liquid becomes deeper coloured, and when a sufficient quantity of alcohol is employed, and the liquor is allowed to evaporate spontaneously, the colouring matter is deposited on the sides of the vessel, in the form of fine needles, sometimes a quarter of an inch in length. The specimens of lichen from which the best crystals of this description were obtained, were from the neighbourhood of Glasgow, and were rather dry, as if they had grown upon a dry wall, little exposed to moisture.

In order to procure the colouring matter of the P. parietina, it is proper to dry the plant at a moderate temperature. This is particularly to be attended to with the sea specimens, which are succulent when compared with the plants from other localities. By this precaution, the alcohol will more effectually extract the colouring matter, without violent or long-continued boiling. We should probably succeed in obtaining the purest product, by removing as much as possible of the water from the lichen, by drying in a stove, and then digesting in cold alcohol. The quantity of the lichen at my disposal has not hitherto been sufficient to enable me to attempt to extract the colouring matter in this manner, but I intend to do so on the first opportunity. I have stated that I have succeeded in obtaining the colouring matter, or *Parietin*, as I propose to term it, in the form of needles, but generally it falls in the shape of brilliant yellow scales, as the alcoholic solution cools. The mode in which I have extracted it was by gently boiling for a few minutes the lichen in contact with the alcohol, then filtering and adding fresh alcohol until the colour appeared to be extracted. The solution has scarcely passed through the filter, before it begins to deposit the shiny scales of *parietin*. If we attempt to purify these by redissolving them in alcohol, we shall find that only a portion is dissolved, and the deposit from the alcoholic solution, instead of presenting the lustre of the substance as at first obtained, assumes the aspect of a brownish yellow powder.

COMPOSITION OF PARIETIN.

The product of the second solution in alcohol, when dried at 212°, and burned with oxide of copper, afforded the following result:—

3.16 grains gave 7.376 carbonic acid.
1.410 water.

This corresponds with

		Expt.	Atoms.	Calcula.	Atoms.	Calcula.
Carbon, .	2.0116	63.65	40	63.82	40	62.51
Hydrogen,	0.1566	4.95	16	4.25	16	4.16
Oxygen, .	0.9918	31.40	15	31.93	16	33.33
	3.1600	100.		100.		100.

As it appeared from the preceding result that the parietin was altered in its character, by attempting to redissolve it in alcohol; the parietin, after being dissolved in alcohol from the lichen, was, after the filtration of the fluid, allowed to deposit by cooling. It was then thrown on a filter, and dried on a tile, and then digested in hot alcohol, to remove any fatty or resinous matter with which it might be contaminated. The same object may be attained by digestion in ether. The *parietin* was then dried at 212°, and analysed.

2.96 grains afforded, when burned with black oxide of copper,
7.15 grains carbonic acid.
1.294 ... water.

This is equivalent to

		Expt.
Carbon, ...	1.9500	65.87
Hydrogen, ...	0.1437	4.85
Oxygen, ...	0.8663	29.28
	2.9600	100.

and agrees with the following calculation:—

		Calculation.	Expt.
Carbon, . .	$.75 \times 9 = 6.75$	65.85	65.87
Hydrogen, .	$.125 \times 4 = .5$	4.87	4.85
Oxygen, . .	$1. \times 3 = 3.0$	29.28	29.28
	10.25	100.	100.

The formula, therefore, will be, according to this view,

$$C_9\ H_4\ O_3\ ;$$

or we may, as in the preceding case, consider it as an oxide of an oil, and the composition, when calculated, would be—

	Atoms.	
Carbon,	40	65.21
Hydrogen,	16	4.34
Oxygen,	14	30.45

and the formula,

$$C_{40}\ H_{16}\ O_{14}\ ;$$

exhibiting a stage in the oxidation of an oil similar to what we meet with in the gradual production of resins from oils of the turpentine type. In some respects the colouring matter under discussion resembles a resin, and especially in its appearance, when precipitated from its solution in alkalies by an acid. If we then consider parietin as a resin, deriving its origin from an oil of the turpentine type, the preceding analyses may be classed as follows:—

Oil of Parietin,	$C_{40}\ H_{16}$
Parietin,	$C_{40}\ H_{16}\ O_{14}$
Oxide of Parietin,	$C_{40}\ H_{16}\ O_{16}$

The effect of reagents upon parietin is striking. A very minute portion of the substance will impart its yellow colour to a large quantity of alcohol, and this solution is sensibly acted on by reagents. When to such a solution a drop or two of nitric, or muriatic or sulphuric acids are added, the yellow colour imparted to it by the *parietin* becomes much heightened, and even a very small proportion (much more minute than that mentioned) will effect a sensible change. When the solution is strong, the addition of acid produces a yellow precipitate. When caustic ammonia, in the smallest quantity, is dropped into, or applied by means of a rod, to a solution of parietin, the yellow colour immediately becomes a rich red, inclining to purple. The same result is obtained with caustic potash, caustic barytes, carbonate of soda, caustic lime, &c.

PARIETIN AS A TEST OF ALKALIES.

The extreme delicacy of parietin in detecting alkalies, suggests its utility in the laboratory. An alcoholic solution may be kept for use, as the addition of a drop or two of the solution to a considerable quantity of an alkaline liquor, will be immediately followed by a change to red; or the process may be reversed, by placing a few drops of the alcoholic solution in a test-glass, and adding to it a drop or two of the alkaline liquor. The alcoholic solution may be prepared simply by digesting the lichen in cold alcohol, of sp. gr. ·840, as I have found that a small portion of lichen will impart a colour to a large quantity of alcohol, sufficiently intense to serve as a very delicate test for alkalies. Observing the strong colour that the alcoholic solution imparted to the filtering paper which was used to purify the solution when first prepared, I cut these into test papers, and found that, when properly impregnated with the solution, they were little, if at all inferior to turmeric paper, in their delicate detection of ammonia. Test paper may be prepared extemporaneously from the alcoholic solution, when it is wished to detect ammonia, by dipping a piece of paper into the alcoholic solution, and then applying it in its wet state to the ammoniacal vapour. The yellow colour is immediately transformed into a reddish purple, but more distinct than the colour that becomes apparent in turmeric paper of old preparation, under similar circumstances, which is a dirty brown. One of the principal recommendations of the liquid test already noticed, is the circumstance of its being capable of preservation without undergoing deterioration, while the test papers which have been frequently recommended although possessing most delicate testing powers when freshly prepared, gradually lose their value by preservation. I believe this to be the explanation of the failure in this country of some continental test papers, which have been recently recommended. It would therefore appear, that the best test paper being that which is of fresh preparation, the most convenient source for its production is that from which it can be most rapidly procured in an efficient state. The observations which have been made upon parietin, in reference to its colouring powers, tend to show that it may be employed with advantage for the most delicate purposes to which turmeric is applied. Parietin, however, is not acted on by acids; the natural yellow colour merely becomes brighter, while turmeric, which contains a blue and yellowing colouring principle, has the former reddened by acids, and the latter converted to a brown by alkalies. Moistened yellow *parietin* paper, on the other hand, becomes red or purple when freshly prepared, and reddish brown, if long prepared, by coming in contact with ammonia and other alkalies. The other reactions of parietin are simple. The alcoholic solution is precipitated yellow by nitrate of silver and acetate of lead, and other metallic salts. A solution of permuriate of iron renders the colour much darker. The precipitates with silver and lead have not been analysed, from the minute quantity of parietin at my disposal.

The yellow colour of the *Parmelia parietina* early attracted the attention of those persons interested in dyes. It was accurately described by Hoffmann, Amoreux, and Willemet, in 1786.* The latter informs us, that the Swedes in the province of Oeland, obtained by means of this lichen and alum a yellow dye for woollen stuffs, and that a flesh tint was also procured from it, fitted for linen and paper; that goats eat this lichen; and that Haller recommended it as a powerful tonic in diarrhœa. He adds, that he had himself used it in his practice as a tisan, and had found it to prove beneficial in that form of the disease which occurs in autumn. Hoffmann states, that in Norway, when boiled with milk, it is used as a remedy in jaundice. This idea may have perhaps originated from the correspondence in colour of the disease and cure, upon the principle so much in vogue at present, "*similia similibus curantur*." Hoffmann affirms that he never could obtain a yellow colour from this lichen, but that with wine vinegar he obtained an olive-green or fawn colour; and with true wine vinegar (*aceto vini vero*) and copperas, a flesh or apricot shade. Of these colours he has appended to his essay specimens, together with forty-nine others, obtained from various species of lichens. Dr John P. Westring of Nordkoping, in Sweden, who prosecuted an extensive inquiry into the colouring matter of lichens, describes the *Lichen parietinus*

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* Memoires Couronnés en l'anneé 1786, par l'Academie des Sciences, Belles Lettres, et Arts, de Lyon, sur l'Utilité des Lichens, dans la Medicine et dans les Arts. 8vo, 1787.

(Wagglaf) as affording, with wool, by infusion for fourteen days, and then boiling for half an hour, a fawn colour; by longer boiling, a yellow was produced, and this mixture became, by simple infusion and extraction, similar to the red wool of Florence. With common salt and nitre boiled for an hour, a beautiful straw colour was elicited. Upon silk it gave similar colours, differing in their shade from red to yellow, according to the methods employed in dyeing the goods.*

Subsequently to these observations, which are perhaps interesting in an economical point of view, the yellow parmelia was recommended by Dr Sande, probably misled by the colour, as a substitute for Peruvian bark, during the last French war. It has also been chemically examined by Herberger, but not apparently with the same results afforded by Scotch specimens, as he found no inorganic constituents which amount to from 6 to 7 per cent., according to my trials, and obtained a much larger quantity of colouring matter than existed in any plants examined by me. He also found a red colouring matter, which did not appear in the process of extraction as followed by me, and which may therefore be a product of the oxidation of parietin. More lately still, Dr Gumprecht extracted yellow oil from the lichen, but in such minute quantity as not to be susceptible of examination. I obtained a quantity of sugar, by means of alcohol, in crystalline grains.

NOTE.—Since the preceding paper was read, the yellow needles described above have been analysed in the laboratory at Giessen, and have been found to consist of $C_{40}\ H_{16}\ O_{12}$, approaching one of the analyses already detailed. So that we have now the following oxides :—

Oil of Parietin, ...	$C_{40}\ H_{16}$
Parietic acid, ...	$C_{40}\ H_{16}\ O_{12}$
Parietin, ...	$C_{40}\ H_{16}\ O_{14}$
Oxide of Parietin, ...	$C_{40}\ H_{16}\ O_{16}$

GLASGOW COLLEGE, *Dec.* 1843.

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* Kongl. Vetenskap, Acad. xii. p. 300, Ann. 1791.

On the Yamúd and Goklán Tribes of Turkomania. By the Baron Clement Augustus de Bode.

Read before the Ethnological Society of London, 13th March 1844, and communicated for this Journal.

In offering an account of some particular branches of the Turkoman race, it will be proper at first to cast a rapid glance over what constitutes Turkomania in general, and name the principal tribes that form the great Turkoman family.

The extensive plains between Bokhara on the east, the Alburs chain to the south, the Caspian Sea to the west, and the Khanat of Khiva to the north, form the natural, although insufficiently defined boundaries, in which the wandering Turkomans roam with their droves of horses and camels, and their flocks of sheep, spreading their tents along the banks of the mountain streams which flow into the Caspian, or are lost in the sands of the desert; and, in default of rivers, digging wells in the dry steppes, to slake their own thirst and supply their cattle, often only with brackish and salt water.

The Turkomans consist of the following great divisions:— The Salú, reckoned the most noble tribe, occupy Serekhs, to the east of Mesched in Khorasan, on the road to Bokhara. The Saruk or Sarik, inhabit Merv at Merú, to the north of Mesched, in a straight line to Khiva. The Tekke, the most numerous tribe, are scattered along the northern skirts of the Alburs chain, called Attók, to the north-west of Mesched, and subdivided into Tekke Akhâl and Tekke Tejen.

The Goklans live to the west of the latter, and the Yamúds to the west of the Goklans, up to the eastern shores of the Caspian.

Before I enter into a more minute description of the Yamúd and Goklan tribes of *Turkomania*, it may be as well to point out the geographical limits to their wanderings. To the west is the Caspian Sea; to the south, the great chain of Alburs and the province of Asterabad; to the east, spurs of the same chain, separating the Goklans and Yamúds from another considerable Turkoman tribe, the Tékéh; and lastly, to the north, the desert extending to Khivah.

This country, the ancient Hyrcania, and very probably the

Vehrkâna, the eighth abode of bliss mentioned in the Zend texts, and known to the Arab writers by the name of *Jurjan;* is watered by two great rivers, the Gúrgan and the Attrek, both of which flow from east to west, and fall into the Caspian Sea. The country occupied by the Turkomans bears the reputation of being healthy. The plain at the foot of the mountains being more open and lighter, than the strip of land which, in Ghilàn, Mazanderàn, and Asterabad, runs between the hills and the shores of the Caspian, there is much less dampness in the air and unwholesome exhalations in summer, which, combined with other causes, produces in those provinces intermittent fever and bilious complaints. The lower courses only of the Gúrghan and the Attrek partake of the same unwholesome climate; there the country is low, and from the overflowing of the rivers in spring, marshes and pools are formed, which, in summer, corrupt the air, breed swarms of gnats, and render the place disagreeable and unwholesome. The heats in summer are tempered by the cool breezes from the sea, which waft freely across the open plains; the dews at night are likewise copious and refreshing; the winter, nearer to the mountains, is not severe; further to the north, in the desert, it is more sensible; there, likewise, the snow lies longer on the ground. Autumn and winter are, however, more especially the seasons for rain, although Turkomonia is not deprived of it at other periods of the year, as is the case on the high table-land of central Persia. Rains are most frequent in the neighbourhood of the hills and near the Caspian Sea.

The Turkomans, then, who live nearest the Caspian Sea, are the Yamúds, having the Goklans to the east of their encampments. As great animosity reigns between them, there is a strip of neutral land which separates the two rival tribes, having the solitary minaret of *Jurfan* as a sign-post to mark the boundary.

The Yamúds are divided into four principal tribes:—

1. Sheref, subdivided into 6 shafts.
2. Chúni, subdivided into 10 shafts.
3. Beyram-Shalí, 5 shafts.
4. Kujúk-Tatár, 8 shafts.

These tribes are said to be the descendants of four brothers, whose father, Yamúd, is looked upon as the founder of their race.

All these tribes encamp on the borders of the Gúrgan and Attrek rivers; extending to the NW., they roam with their herds in summer in the hilly country of Balkhan, and many families are settled in the Khanát of Khiva. The average number of the Yamúds amounts probably to 40,000 or 50,000 families. The principal distinction among the Yamúds is their division into *Chomúr* and *Chorvá;* it is founded on the difference of their mode of occupation, and the relative distance of their encampments in respect to the Persian territory of *Asterabad.*

The Yamúd Chomúr occupy both the banks of the *Gúrgan* river, and even stretch as far south as the river Karasú, where they have their corn-fields, their rice plantations, and vegetable gardens. They are less wild than their neighbours to the north, the *Chorvá;* give themselves up to agriculture, and are on much better terms with the Persians than the latter. They often visit the bazars of *Asterabad*, bringing into the market for sale the produce of their industry—such as felt, and woven carpets, wheat, (which is much superior to that of Asterabad), barley, butter, sheep, horses, &c., and receive in exchange the coarse manufactures of that province, consisting of different sorts of *alijeh* or silk stuffs from *Anezane* and other districts, *kadek* or cotton from the looms of *Shahrúd*, in Khorasan, of Burujird, (near Hamadan), and of Isfàhan. This friendly commercial intercourse with their neighbours does not prevent them from committing plunder whenever a favourable opportunity offers; but the principal charge lodged against them by the Persians, is, that they give refuge and screen from pursuit the foraging parties of the Attrek Yamúds, in their incursions into the territory of Asterabad.

The Yamúd Chorvá encamp to the north of the former, on the banks of the river Attrek. They are the same Yamúds as the Chomúr, composed of the same tribes; the only difference is the mode of life they lead, which is essentially pastoral; they have more numerous flocks of sheep, herds of camels, and droves of horses, than their agricultural neighbours of the Gúrgan river, and, living nearer the desert, and further from Asterabad, they are perfectly independent of the Persian sway.

It frequently happens that the Chomúr and the Chorva change their avocations. When a Chomúr realises a small fortune, he lays out his stock in the purchase of sheep, camels, &c., quits the banks of the Gúrgan, approaches the desert, and becomes a Chorva, in order to be beyond the reach of the Asterabad authorities. On the other hand, when a Chorva is deprived, through misfortune, of his flocks, he turns agriculturist, and becomes a Chomúr. According to the principles laid down in political economy, the agriculturist stands a degree higher in the scale of society than the shepherd who tends his flocks; it is the reverse on the plains of Turkomania. We must observe, however, that although the Chorva lead essentially a pastoral life, they still possess some fields which they cultivate between the rivers of *Attrek* and *Gúrgan,* but the soil is much inferior to that on the southern banks of the latter river.

It would be foreign to the object of the Society were I to dwell at any great length on the various remains we meet on the plains of the Yamúds and Goklan Turkomans, and which denote that this country must have been formerly densely populated, and have attained a certain degree of civilization, to which, at present, it can lay no claim. I shall, therefore, limit my observations to a summary sketch of what may appear most interesting. The first object deserving of notice is the great wall which runs from E. to W., and situated between the Gúrgan and Attrek rivers. By whom was it erected? In D'Herbelot we find some obscure accounts about a certain wall to the east of the Caspian, which he surmises may extend to the Chinese walls, and compares with the *Saddi Tuj-i-Majuj* of the Arab writers—the Gog and Magog of Scripture. Oriental historians speak of a wall which Núshirvan raised against the encroachments of northern barbarians, or only repaired an old one, attributed to Alexander Dúlkarneïn; but I shall leave the hypothetical and historical part concerning this wall (although the subject well deserves a closer investigation), and attend to the description of it in its present state.

The wall commences at the mountain of *Pushti-Kémer*, about fifteen miles below the source of the Gúrgan river, along which it is carried nearly in a parallel line with the stream, on the right bank, till it reaches the shores of the Caspian, and the continuation of it is said to be seen under water for some distance. The whole length of the wall may be ninety miles, or thereabouts. I must observe, however, that it does not form an uninterrupted rampart, but consists of mounds of various heights—in some parts from eight, to ten or twelve feet high, in others level with the ground, and imperceptible. The wall, externally, is covered with earth, and overgrown with grass and brambles; and it is only from the intersections, and the bricks strewed about, one can judge that it was built of large square bricks.

At fixed intervals there are square redoubts, each face of the redoubt measuring 150 paces; on some of them the Turkomans have their burying-ground.

This wall goes by the name of Kizil-Allán, (Kizil meaning gold, and Allan probably borrowed from the once powerful nation of that name, who were settled for a time near the Caspian, and who, during the rush of the Asiatic hordes into Western Europe, penetrated, with the Suevi and the Vandals, into Spain.) Klaproth is of opinion, that the Ossets, spread in the Kabarda and the valleys of the Caucasus, are a remnant of the Allans. Deguignes says nearly the same thing.

Beyond the Kizil-Allán is another wall, running parallel to it, but much lower, and in many places imperceptible. The intervening ground forms a road at present; but I suspect that formerly it must have been a canal or ditch, serving two purposes—that of strengthening the line of defence, and, in time of peace, supplying water to the fields beyond the Gúrgan. What bears me out in this conjecture is the following fact:—As the Gúrgan, with its tributaries, flows between very high banks, water could not be procured for irrigating the fields; the cultivators of the land were obliged, therefore, by means of canals, to bring the water from a distance, where its level was higher than the fields which required irrigation. I met with several of such canals, brought from the mountains, and extending to the Gúrgan, with remains of aqueducts, by means of which the water was carried over the river, and then ran along another channel, till the junction of the latter with the *Kizil-Allán* wall, which in those parts is cut across, to allow the passage for the water.

The tower called Gúmbet-i-Kabús has been described by other travellers; it stands amid the ruins of the once populous town of *Jurjan*, celebrated for its learned men, and capital of the whole province which bore its name, now covered with high grass and reeds—a receptacle for leopards and other beasts of prey.

Of the town of Bibi-Shirvàn nothing more is seen beyond a number of green mounds, although the Turkomans assured me that some deep subterraneous passages have been discovered there.

Both Bibi-Shirvàn and Jurjan are said to have been destroyed by an earthquake. I likewise learned, when it was too late to retrace my steps, of the existence of a great reservoir of water, somewhat in the style of the Lake Mœris, in Egypt. The natives call it *Ystákhl*, and say that it is about 7½ English miles in length, and wide in proportion, and above 30 feet in depth. In spring, the water of the torrents, flowing from the mountains, is accumulated in the lake, and is used in summer for the irrigation of the rice plantations.

Gaúr-Kaléh, Perez, Sháhrek, appear to have been towns or fortified camps. Dashtalghé or Salocil represents, in all probability, the site of the palace and pleasure-gardens which Amir-Timur had constructed for the ladies of his harem during the winter he spent at Turjan, which is close by.

The plains of Turkomania possess above 60 very considerable artificial mounds, such as are met in Khorassan; they certainly are of great antiquity, and may be referred to the times of the Scythians or Parthians. In one of these mounds some very curious articles, in gold, copper, and marble, have been lately discovered. A detailed description of them has been presented by me to the Society of Antiquaries.

The ruins of Ak-Kaléh, a modern town, are situated between the Kara-Sú and Gúrgan rivers. It was once the capital of the Kajars.

The Goklans ascribe their origin to two brothers, *Du-*

dargá and *Alghidagli*; from whom all their different clans proceed. They are now divided into the following tribes:—

1. Yangakh.
2. Senkrik.
3. Kerrik.
4. Boïnder.
5. Kara-Balkhan.
6. Erkegli.
7. Koïi.
8. Ay-dervish.

The number of the Goklans formerly amounted to 12,000 families, but of late years it has, from various causes, considerably decreased. The Khan of Khiva forced several thousands to settle in his dominions; others voluntarily migrated in the same direction at the approach of a Persian army in 1836. They soon found, however, that they had exchanged their wooded, fertile, and beautiful valleys, abundantly watered by mountain streams, for a barren and sandy waste, and prepared to return to their former habitations, but were met with an obstinate resistance on the part of the Khan of Khiva, who, to prevent their escape, issued an order that the first deserter should be thrust into the mouth of a loaded mortar and blown up into the air. But such is the love of country, and the power of local associations, that the Goklans braved the sanguinary decree, and the no less dangerous flight across the desert; and many escaped pursuit. It is on occasions like those that the excellency of the Turkoman horse can be best appreciated—the fugitives being obliged to traverse, day and night, immense tracts without water. The Goklans, on escaping from the pursuit of the Usbeks, are exposed to the attacks of the Téké Turkomans, their deadly foes, through whose territories they are under the necessity to pass before they can reach their native vallies. Hiding themselves during day time in ravines, they continue their flight at night, often traversing from 35 to 40 miles at a brisk trot on their hardy, yet slender-limbed animals.

As a proof (if proof be yet necessary) how kind Providence is ever watchful to help the needy, I may state that, on the very verge of the desert, but still in the country of the enemy, the Goklans find a tribe of their own countrymen—the *Koï*—

settled there for a number of years by permission of the Téké Turkomans, who never molest them. Here the fugitives alight for a short time, to fetch breath, and snatch a momentary repose: the Koï procure them food, and often fresh horses, to continue their journey.

The beacon which serves to guide the Goklans across the dreary steppe is the snowy peak of Demarend (not more than 30 or 40 miles from Teheran), and yet seen at a great distance in the desert of Khorazm.

The Turkomans follow the creed of Mohammed, and are of the Sunni persuasion, *i.e.*, they recognise the four caliphs, his immediate successors. Although not very scrupulous in following the tenets of the Koran, they still have their Mullahs or Cazi, the propounders of the law, who, at the same time, are the civil judges of the tribe. These Cazi follow their course of studies at the colleges at Khiva, but are seldom less ignorant than the rest of their countrymen, although perhaps more crafty.

Among the Turkoman tribes there are four distinct from the rest, supposed to be descended from the four first caliphs. They are equally respected by the Yamúds, the Goklans, the Tekke, the Salúrs, and the Sariks, and are not touched by those rival tribes.

The names of these four families are the following:—The Khoja, descendants of Ali; the Atta, descendants of Omar; the Shikhs, descendants of Osman; and the Makhtum-Kúlí, descendants of Abúbekr.

Some of these tribes have turned to account the advantageous position they enjoy amid their countrymen; and, under the safe-guard of their sacred origin, have become merchants, traversing with their caravans of camels in all the directions of the desert—carrying goods to the different hostile tribes, and receiving others in exchange, without fear of being plundered or molested by any of them.

In wishing to present here a sketch of the Turkoman character, I regret that I can find but very few redeeming qualities to palliate the evil propensities of their nature. The Turkomans are said to be brave; but I am inclined to suspect that this notion arises from the circumstances of their enemies being cowards. A Turkoman feels always reluctant to expose himself to danger; his warfare against the Persians is seldom a manly, open war; it generally consists in sudden unexpected incursions. The Turkomans approach the Persian villages in stealth, in the dead of night, wait for the first dawn of morning, then rush on the disarmed and drowsy population, plunder what they can; after which they retreat precipitately, carrying off into their deserts the captives who have fallen into their hands. Their piratic exploits on the Caspian are likewise directed against the poor villagers of Mazanderan, who venture too near the beach, or fish in the sea. If the Yamúd finds his enemy armed with a matchlock he seldom attacks him, but speedily retreats, or hides himself in the thick forest. The Turkoman seldom makes use of fire-arms, and prefers the lance and the sabre.

The ruling passion of the Turkoman is thirst for plunder—nothing is reckoned sacred that stands in the way to the attainment of the objects of his cupidity; and when force cannot be employed, he has recourse to cunning in order to be possessed of the object he covets.

The second passion which fills his breast is revenge: it is subservient to the first, and proceeds generally from some sordid motive. The secret, and often the ostensible, cause of their bloody feuds, is founded on the prospect of plunder.

The reason why the Turkomans are more inhuman than the other barbarous wandering tribes, cannot be attributed, I believe, to any other cause, than that they are slave-dealers. Their daring forays are usually undertaken with a view to carry away captives, whom they retain in chains until redeemed by their relations, or sell them in the bazars of Khiva, if the ransom money fails to arrive in time, or proves insufficient to satisfy their cupidity. The prisoners are sometimes retained for their own use, and sent to tend their flocks in the desert, or employed in field works. Thus they are the terror of their neighbours—the Persians of Mazanderan, Asterabad, and Khorassan, who are obliged to be always on their guard against the sudden attacks of the Turkomans.

As the Persians are of the Shia sect, and the Turkomans of the Sunni, the latter justify themselves on the ground that to seize on a Persian and sell him is lawful; others, however, are more sincere, and own that if the Persians, instead of

Shia, had been Sunni, then they themselves must have turned Shia,—as the circumstance of being of the same religion might have interfered perhaps with their present lucrative trade. These religious scruples do not prevent them, however, from capturing persons of their own religious persuasion and their own tribe, with whom they happen to be in enmity, and fixing enormous prices for their release.

If this thirst for gain renders the heart of the Turkoman callous to the suffering of his fellow-creatures, I found, on the other hand, that the feelings of sympathy are more developed among the inhabitants of Asterabad, than in other parts of Persia,—the danger apprehended from the Turkomans being the tie which unites them. Thus, if any of them falls into the hands of the enemy, subscriptions are made to release the captive, and the whole community takes a lively interest in the sad event. I beg leave to mention here an instance, of which I myself was a witness.

On riding one evening through the streets of Asterabad, I found a woman kneeling in an open mosque, clinging with both arms to the pulpit, and weeping bitterly. On inquiry, I found that intelligence had just been received of her son being kidnapped by the Turkomans. I recommended the disconsolate mother to pray with faith, and God would hear her prayer. In the meanwhile, the news spread through the town, horsemen were sent in pursuit of the robbers, but returned without having discovered any traces of them. The boy, a lad of 13, was the son of a common dyer, and had strolled out of the gate with a companion to fetch fuel from the wood close to the walls. A Turkoman, it seems, who had been prowling like a beast of prey, seized on him, while the other lad made his escape.

The night was far advanced, when a loud noise in the street close to my dwelling aroused our party. It was the lad, who had made his escape from the Turkoman, and was now led in triumph about the streets with joyful acclamations. As I had evinced some interest in the catastrophe, his father brought him to shew me that he was safe.

It appears that the Turkoman, while it was light, lay hid in the thicket; as night came on, he issued from his hiding place, and stole near to the ramparts of the town, in order to gain the plain, dragging along with him his captive by the arm, who dared not scream for fear of being put to death. He, however, recollected that he had a knife in his right pocket, and complaining that his right arm was quite benumbed from the Turkoman's grasp, he entreated him to release it for a while and take hold of the left; to which the robber acceded. As soon as he had found his right arm free, he thrust it into his pocket, seized the knife, and with all his force hit a blow on the hand which held his left arm. The man let go his hold from pain, and the boy dashed into the thicket. The night was dark, the town too near for the Turkoman to tarry long; he soon gave up the search, and fled to the Gurgan river, while the boy ran to the city-gates, and knocked for admittance.

The Yamuds, as well as the Goklans, have a very high opinion of their own race, and never grant their daughters in marriage to strangers foreign to their respective tribes, like the Rajpûts in India.

To prove how great is their susceptibility on this point, I shall state a fact which took place during the reign of the late Feth Ali-Shah.

Mirza-Naghi-Khan, of Fenderis, father to Mir-Sadûllah-Khan, the present chief of this district, fell in love with a young Turkoman girl, and demanded her in marriage from her parents. They resisted for a long time, but at last, by money and fair promises, yielded to his importunities, and their daughter became his wife. This event exasperated greatly the whole tribe against Mirza-Naghi-Khan; but as he was a powerful and dangerous neighbour, they stifled their feelings, made peace with him, and feigned to have forgotten the affront. At the expiration of a year, the young Turkoman woman expressed a wish to visit her parents, and as the Persian Khan felt no apprehension in her going, he granted her request. But no sooner had she entered the encampment of her tribe, than the Turkomans seized on her, dragged her to the top of an artificial barrow, and there, in the presence of her parents, cut her to pieces. Foreseeing the vengeance which threatened them on the part of Mirza-Naghi-Khan, and not feeling themselves sufficiently strong to resist him, the

Turkomans broke up their tents and retired to Khiva. But if we feel shocked at the barbarous act above related, how much more shall we have to deplore the atrocious means to which the injured party had recourse in retaliation for the deed?

Mirza-Naghi-Khan made believe that he was sorry for what had happened, acknowledged himself in the wrong for not having respected their prejudices, and pledged his word that no harm should be done to them, if they would only return. The Turkomans believed him; but they were not reinstated long in their former encampments, when Mirza-Naghi-Khan seized an opportunity to fall on them unawares, and carry away about 50 women of their tribe, whom he put to death in cold blood, in order to avenge the death of his wife, and slake his thirst for vengeance. The decrees of Providence are ever just,—a few years later the chief of Fenderiss himself fell by the hands of the Turkomans.

The Turkomans observe a difference between their children from Turkoman mothers, and those from the Persian female captives whom they take as wives, and the Kazakh women whom they purchase from the Uzbeks of Khiva. The Turkomans of pure race enjoy full privileges, while the others are not allowed to contract marriages with Turkoman women of pure blood, but must choose themselves wives among the half-castes and Kazakh captives.

As there exists a great animosity between the Yamúds and Goklans they do not intermarry, although they reckon themselves of equally noble lineage. The same hatred is extended to the Tékké Turkomans, whom the Goklans and Yamúds, moreover, look upon as their inferiors, being, according to their genealogies, the descendants of a slave-woman, whilst they are the posterity of a free-woman.

All subjects are better understood when explained by comparison; faithful to this principle, I shall endeavour to delineate the physical features of the Turkomans, by likening them to, or distinguishing them from, the Mogol race, as a term of comparison, because their exists some affinity between them. The eye of the Turkoman is formed on the same principle as that of the Mogol, and appears to constitute a remarkable feature in the Mogol race. It is the eye of the feline species, with the extremities drawn up towards the temples; but, if I am not mistaken, the pupil of the eye is not so black with the Turkoman, and the eye larger. Neither is the nose so flat, nor the lips so thick, although the high cheek-bones bear the Mogol type. The Kalmuk approaches nearest to, or is more probably identical with, the Mogol; he has the same low forehead, the head pressed down, forcing the cheek-bones to protrude forward; the same flattened nose, and thick pouting lips, with small black eyes, nearly hid from sight by his swollen face; the same jet black hair; the chest is likewise broad and muscular; and, to judge by appearance, one would think it alone endowed with power at the expense of the lower part of the figure, as the legs are short and gauky; but one is brought to form a better opinion of them, when the Kalmuk vaults on horseback, without saddle or bridles clinging fast to the sides of the animal with his thighs and, ankles, and defying the wildest horse of the steppe to throw him down. The Turkoman does not resemble the Mogol in these respects:—He has a high forehead; his hair is not so black; the chest less developed, in fact, narrow and flat like that of the Persian race; or, bringing the comparison nearer home, like the chest of his own breed of horses. Like his noble animal, the Turkoman, generally speaking, is tall, well-shaped, with large bones. He is not deficient in strength, and has muscular arms, probably from the use of the bow; but the arms of the women are perhaps still more muscular, owing to the heavy work which falls to their share.

As the Turkomans generally wear long flowing robes, I could not well examine the form of their legs. There appears, it is true, a certain curvature, by the toes being bent inward, which may proceed from their equestrian habits from childhood; but they are not so bandy-legged as the Mogols or the Kalmuks.

If I were to search for a family resemblance between the Turkomans and any other Turkish tribe which has fallen under my notice, I should be inclined to compare them to the Nogay-Tartars, in Northern Daghestan, on the Western shores of the Caspian. The Nogay, with the Krim, the Astrakhan, and the Kazan Tartars, formed once the Golden Horde, under the sway of Mogol or Tartar Chiefs. It was in consequence of the Mogol dominion that the name of Tartar extended to them, although they reckon themselves of the same extraction as the Turks of Constantinople; and the Turkomans pretend to be of the same origin. It is affirmed, however, by those who have studied the several Turkish dialects, that there exists a material difference between the language spoken at Constantinople, and that by the Kazan Tartars, or the Turkomans, who, together with the Usbeks and others, speak the Jagataï Turkish. The language in use among the wandering tribes of Turkish origin in Persia is different from both the former, and is reckoned a corrupted dialect. That of Constantinople is the most elegant and the best cultivated of the three.

The more intimate connection of the Astrakhan and Kazan Tartars with the Mogols can be traced in their features; with the *Nogay* it is less visible. In like manner, the Turkomans further off in the desert, and the Uzbeks of Khive, have more of the Mogol expression than the Turkomans who encamp near the Persian frontier. The frequent intercourse of the Nogay, in latter years, with the Cherkess, seems to have improved their race; and notwithstanding the enmity that exists between the Turkomans and the Persians, it is still not unlikely that their close vicinity should have produced on the former a similar effect in a lapse of several centuries. The fact we have seen, that the Turkomans marry Persian women, when they take them as prisoners. The Turkoman women are, like the men, tall, and when young, well-shaped; their faces are rounder than those of the men; the cheek-bones less prominent; the eyes black, with fine eye-brows, and many with fair complexion; the nose is rather flat; the mouth small, with a row of regular white teeth. In a word, a great number of the younger part of the community might be reckoned as fair specimens of pretty women.

I hope I may not be accused of partiality if I do not draw an equally advantageous picture of the old Turkoman matrons; for they are downright hideous, to say the least of it. Their ugliness is, however, cast in a different mould from that of the old women among the wandering tribes of Persia. The latter have sharp-marked features, with a wild piercing eye sunk in a hollow socket; the face of the former, on the contrary, is nearly flat, with hardly any appearance of a nose, and shrivelled all over.

What presses the nose of the Turkoman women towards the top lip, is their custom of hiding their mouths under a handkerchief, which reaches to the tip of the nose, pressing it down. The same custom prevails among all the Armenian women in the East, and is reckoned as an indispensable condition of female decency. This part of the dress is somewhat similar to the *Penom* worn by the ancient Gebber priests whenever they approached the sacred fire, for fear their breath should pollute the pure essence and symbolic manifestation of the Deity; for, according to the doctrines of the *Zend-Avesta*, the same as in Gospel truths, it is that which cometh out of man which defiles him; with that difference, that Zoroaster understood it in a more literal sense.

We have seen that the Yamuds are addicted both to a pastoral and an agricultural mode of life—although more especially to the former. Their neighbours, the Goklans, are more settled. Their tents are spread in beautiful valleys, others in the plain along the banks of the Gurgan and its tributaries. Their chief occupation is agriculture; the land is reckoned very productive, although at present much neglected. The soil between the mountains and the Gurgan, consisting in black earth and clay, is used under wheat and barley fields,—the crop of which, in proportion to the seed, in good years, is 100 to 1. Beyond, but close to, the river Gurgan, the fields yield the sixtieth grain, and less as one advances towards the north. We might feel somewhat reluctant to admit this great disproportion between the seed and the harvest, were we not informed by Herodotus, on whose veracity we may safely rely, that the fields near Babylon produced corn in the proportion of 200 bushels to one bushel committed to the earth.

Independent of field work, the Goklans have plantations of the mulberry-tree for their silk-worms. If China be the fatherland, as it is supposed, of the silk-worm, then in travelling towards the west, this insect was probably reared in the valleys of Gurgan, before it spread and flourished in the provinces of Ghilan, of Shirvan, or even attained Asia-Minor and Brusa.*

We read in the Arab writers, that in the flourishing days of Jurjan the revenue of the province was collected in raw silk.

The Turkomans marry their children at an early age—the lads from fourteen to fifteen, and the girls from ten to twelve. But in cases of early marriages a singular custom exists among them. After the ceremony is over, the young spouse tarries only two or three days with her youthful mate, and then returns to her parents, where she remains two, and sometimes three, years. During this interval she prepares her dowry consisting in her apparels, and the necessary articles for adorning the interior of their future tent. When the two or three years are completed she is conducted to the tents of her father-in-law, and lives there with her husband during a twelvemonth. At the expiration of the year, the father allows his children to have a separate household, especially if a child be born unto them. Separate tents are then allotted to the young couple, and the young man receives his share of his father's property, consisting in camels, horses, flocks of sheep, &c. But notwithstanding the separation has taken place, the father still continues to provide for their maintenance the first six months; after which the young man becomes free from the control and guardianship of his parent, who till then had exercised an unlimited power over him—possessing the right even of life and death, without being liable to give any account of his actions to the society of which he is a member.

As hard work generally falls to the lot of the women, while the men saunter away their time, when not engaged in a foray, the Turkomans prefer young widows to young girls for their wives, as the former are more accustomed to hard labour, and more experienced in household concerns. The Turkoman widows fetch a double price in comparison to spinsters. Thus, if a girl be worth the value of five camels, a widow cannot be had under the rate of ten camels. But justice must be given to these women for their industrious habits; they have always some work in hand, and are seldom seen idle or loitering about; and notwithstanding the fatigues of the day's labour, I generally found that whenever an enemy was prowling about the camp at night, they were always the first, like the geese of old Rome, to give the word of alarm.

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* See in Professor Ritter's geographical work, entitled "*Erdkunde*," an interesting historical account of the culture of the Silk-worm.

The Turkomans have a notion that they cannot shew a greater degree of respect to their departed friends than by burying them the moment they give up the ghost; and it is greatly to be feared that many an unfortunate victim is thus prematurely hurried to the grave from this mistaken notion of honouring the dead.

On the spot, in the field where the corpse is washed, the Turkomans raise a small barrow, and dig a ditch round it; from thence the dead body is carried to the burying ground of the tribe on some elevated tepeh or artificial hillock, as there are so many on the plains of the Turkomans since times immemorial. As soon as the sad news spread abroad, all the relations and friends arrive from the different encampments to condole with and offer consolation to the family of the deceased. They bring their own tents and place them in a circle round that of the mourners; the women then go by turns to weep with the family, especially such who have gained some reputation in the art and manner of weeping in the proper style. The men remain mostly out of doors; and, as on all occasions when they meet, whether at joyous festivals or funeral ceremonies, riding is their great amusement, horse-races are usually resorted to. They remain, thus carousing, for weeks together, at the expense of their host, until the latter announces to his friends, that, thanks to their endeavours, he (if it be a widower) or she (if a widow) feels consoled and resigned to his or her fate. This is a signal for the party to break up their tents, and take their leave. These condoling visits become very expensive to the poor survivors, who have often not only to weep for the loss of their relative, but likewise that of being ruined into the bargain by their considerate friends. It sometimes happens that rich Turkomans, from a feeling of ostentation, retain their guests for a whole month, feeding them with rice, mutton, cheese, butter, milk, and such other produce of their flocks and herds, in which their riches chiefly consist. Among the Yamuds, there are individuals who possess upwards of 1500 sheep, 200 camels, from 20 to 30 mares, and as many captives. The Goklans are not so rich in herds. I have mentioned that the Turkomans are fond of horse-racing. It is quite a passion with them. To a Turkoman a horse is everything. On its strength and power of endurance depends materially the success of his predatory excursions into the enemies' country; on its fleetness—his means of escape. It is to develope these essential qualities that the Turkoman consecrates to his horse all his leisure hours. To say that he attends more to the care of his horse than to his own child, would not be saying much; because the latter is left completely at the mercy of chance, to grow up as he can, while the favourite horse receives all the attentions, not only of its master but of the whole family. It would take us too far were we to enter into the details of training Turkoman horses; moreover, other travellers have already given descriptions about it; we shall only observe that the Tekke are reckoned the best horses for a long and protracted journey and forced marches; *the Goklan and Yamud are more slender, and swifter horses.*

The Tekke are preferred even to the pure Arab blood-horse, by the Persians at the Court of the Shah, and among the great men. As the encampments of the Tekke Turkomans are among the ruins of Nissa, it is very probable that the Tekke horses belong to the same *Nissean* race of horses which Strabo, and other ancient writers, mentioned as being mostly prized by the Persians. It is equally to be supposed, that it was on the same Turkoman breed of horses that the Scythians, and, later, the Parthians, waged war against their enemies; and the plains of Turkomania was the seat of their dominions.

When not engaged in plundering expeditions, nor exercising their horses, the wandering Turkomans lead an idle life, spending the day in sauntering from one tent to the next. They assemble in groups, and find great pleasure in talking over their deeds of prowess, and cunning manœuvres in surprising their enemies. Among other recreations, we must not omit to mention that the Turkomans are very fond of the game of chess, and are reckoned to be great proficients in it: even their enemies, the Persians themselves, who are good chess-players, admit the superiority of the Turkomans in that respect. What renders the game more puzzling, and the calculations much more difficult, is, that their chess-board is not, like ours, divided into thirty-two light and thirty-two dark compartments for the movers, but consists all of one colour. It is nothing more than a four-cornered linen rag, with lines drawn over it in a vertical and horizontal direction to mark the sixty-four compartments. This simple chess-board, which can be wrapped up as a pocket-handkerchief and carried about in the pocket, is evidently manufactured by their women, for the transversal lines are stitched on the linen with dark worsted threads. It is recorded that, during the reign of the late Shah, a Turkoman came to Teheran, and having been admitted into the presence of the Feth-Ali-Shah, he beat all the best chess-players at the Court of his Majesty, and gained a large sum of money.

NEW PUBLICATIONS RECEIVED.

1. Elements of Natural History, for the use of Schools and Young Persons. By Mrs R. Lee (formerly Mrs T. Edward Bowdich). 1 vol. pp. 485, with engravings in wood. Longman and Co., London. 1844. *We recollect no work on Elementary Zoology more deserving of introduction into our schools than this pleasing volume, by a lady already favourably known to the scientific world.*

2. Researches on Light. By Robert Hunt, Esq., Secretary to the Royal Cornwall Polytechnic Society. 8vo, pp. 303. 1844. Longman and Co., London. *Mr Hunt's reputation is so well established, that we need only mention this volume to secure it a favourable reception from the philosophical public.*

3. Fifth Annual Report of the Registrar-General in England. 2d edition, revised and corrected, 8vo, pp. 603. London, printed by W. Clowes and Sons, Stamford Street. For Her Majesty's Stationery Office. 1843.

4. Transactions of the American Philosophical Society, held at Philadelphia for promoting Useful Knowledge. Vol. ix. New Series, Part 1st, 4to. Philadelphia. 1844. This part contains three articles :—Article 1st, Continuation of Mr Lea's Paper on Fresh-water and Land Shells. 2. Trigonometrical Survey of Massachusetts. 3. Observations on Egyptian Ethnography, derived from Anatomy, History, and the Monuments. By Samuel George Morton, M.D. Illustrated by an extensive series of Engravings. *This important memoir we shall notice in our next number.*

5. Die ersten Begriffe der Mineralogie und Geognosie fur junge practische Bergleute. Von Friederick Mohs. Herausgegeben nach Seinem Tode. 2-Bänd, 8vo. Wien, 1842. *A copy of this work has at length reached us. The second volume—that on Geognosy,—we consider very interesting, and likely to change or modify prevailing views in Geognosy and Geology.*

6. A System of Mineralogy, comprehending the most recent discoveries, with numerous woodcuts and four copperplates. By James Dana, A.B. 2d edition, 1 vol. 8vo, pp. 801. New York and London, Published by Wiley and Putnam. 1844. *This beautiful volume does great credit to the author, and also to his printers, engravers, and publishers. We have carefully examined it, and although we do not agree with him in all his views, we can with great truth recommend his book to every one desirous of becoming acquainted with mineralogy, as one of the best treatises on this very important branch of Natural History in our language.*

7. A Lecture on Institutions for the Better Education of the Farming Classes. By Charles Daubeny, M.D., F.R.S., Professor of Rural Economy at the University of Oxford. 8vo, pp. 32. John Murray, London. 1844.

8. Lehrbuch der Naturphilosophie. Von Oken. Dritte, neu Bearbeitete Auflage. Zurich, 1843. *This work contains a condensed view of the celebrated author's speculations in general natural history, and his systems of geology, mineralogy, zoology, and botany.*

9. Lehrbuch der Physikalischen Geographie und Geologie. Von B. Studer, Doctor and Professor in Bern. Erstes Capitel Enthaltend: Die erde im Verhältniss zur Schwere. 8vo, pp. 398. Bern, 1844. *Much is expected from this work on Physical Geography, by a philosopher so celebrated as its author. The first part only has reached us. We therefore delay expressing our opinion of its merits until the whole work is before the public. We may, however, remark, that Geologists look forward to the volume on Geology as likely to contain a full geognosy of the Alps, which Professor Studer is so able to give, from his very extensive practical acquaintance with them.*

10. Excursion through the Slave States. By G. W. Featherstonhaugh, F.R.S., F.G.S. 2 vols. 8vo. John Murray, London. *These amusing and interesting volumes we recommend to the attention of our readers. The author's Geological observations will be considered afterwards.*

11. Grundzuge der Botanik Entworfen. Von Stephan Endlicher und Franz Unger. 8vo, pp. 494. Wien, 1843. *This is one of the best of the smaller philosophical works on Botany we are acquainted with.*

List of Patents granted for Scotland from 22d March to June 1844.

1. To **William Ritter**, of 106 Fenchurch Street, in the city of London, gentleman, being a communication from abroad, "improvements in crystallizing and purifying sugars."—26th March 1844.

2. To **Charles Harrison**, manager of the Coed Talon, and Leeswood Iron-works, Flintshire, "certain improvements in the manufacture of cast-iron pipes, and other iron castings."—26th March 1844.

3. To **William Isaac Cookson**, of the borough and county of Newcastle-upon-Tyne, Esquire, "improvements in apparatus for burning sulphur in the manufacture of sulphuric acid."—26th March 1844.

4. To **Elisha Haydon Collier**, of Goldsworthy Terrace, Rotherhithe, in the county of Surrey, civil engineer, "certain improvements in the construction of furnaces and flues."—27th March 1844.

5. To **Joseph Dickenson Stagg**, of Middleton in Teesdale, in the county of Durham, manager of smelting works, "a new or improved plan for collecting, condensing, and purifying the fumes of lead, copper, and other ores and metals; also the particles of such ores and metals arising or produced from the roasting, smelting, or manufacturing thereof; and also the noxious smoke, gases, salts, and acids, soluble and absorbable in water, generated in treating and working such ores and metals."—30th March 1844.

6. To **William Edward Newton**, of the Office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer and patent agent, being a communication from abroad, "an improvement or improvements in furnaces."—4th April 1844.

7. To **John Stevelly**, of Belfast, in the county of Antrim, professor of natural philosophy, "improvements in steam engines."—10th April 1844.

8. To **Thomas Nash**, of Paul's Cray, in the county of Kent, paper-maker, and **Francis Pirie**, of Watling Street, in the city of London, paper-maker, "certain improvements in the manufacture of paper, and in the machinery to be used therein."—11th April 1844.

9. To **William Thomas**, of Cheapside, in the city of London, merchant, being a communication from abroad, "improvements in fastenings for wearing apparel, and which may also be applied as fastenings to portmanteaus, bags, boxes, books, and other things."—15th April 1844.

10. To **John Lawson**, of Leeds, in the county of York, engineer, and

Thomas Robinson, of Leeds, in the county of York, flax-dresser, "certain improvements in machinery, for heckling, dressing, combing, and cleaning flax, wool, silk, and other fibrous substances."—17th April 1844.

11. To **John Lee**, of Newcastle-upon-Tyne, Esquire, "improvements in obtaining products from sulphurets and other compounds containing sulphur."—24th April 1844.

12. To **William Scott**, of Belton Street, Piccadilly, in the county of Middlesex, Esquire, being a communication from abroad, "improvements in the manufacture of fuel."—24th April 1844.

13. To **William Henry Barlow**, of Leicester, civil-engineer, "improvements in the construction of keys, wedges, or fastenings, for engineering purposes."—24th April 1844.

14. To **John Dixon**, of Wolverhampton, iron-master, "improvements in heating air for blast-furnaces, and for other uses."—26th April 1844.

15. To **William Wright**, of Duke Street, St James's, in the county of Middlesex, surgeon, "certain improvements in rendering leather, skins, or hides impervious to wet, more flexible, and more durable."—30th April 1844.

16. To **John M'Intosh**, of the city of Glasgow, in Scotland, gentleman, "certain improvements in revolving engines, and an improved method of producing motive power, and of propelling vessels."—30th April 1834.

17. To **Samuel Faulkner**, of Manchester, in the county of Lancaster, cotton-spinner, "certain improvements in machinery or apparatus for carding cotton and other fibrous substances."—30th April 1844.

18. To **William Irving**, of No. 102 Regent Street, Lambeth, in the county of Surrey, "improved machinery and apparatus for cutting and carving substances to be applied for inlaying and other purposes."—3d May 1844.

19. To **James Murray**, of the Garnkirk Coal Company, in the parish of Cadder and county of Lanark, Scotland, "a new method of using and applying artificial gas made from coal, oil, or other substances, for lighting and ventilating caverns, pits, or mines, or other pits where minerals or metals are worked or extracted."—3d May 1844.

20. To **James Bremner**, residing at Pulteney Town, in the county of Caithness, civil-engineer, "certain arrangements for constructing harbours, piers, and buildings in water; for cleansing harbours, and for raising sunken vessels."—9th May 1844.

21. To John Wilkie, of Glasgow, mechanic, "improvements in machinery or apparatus for working wood into the various forms required for making doors, window-shutters, window-sashes, mouldings, flooring, and other purposes."—16th May 1844.

22. To Frederick William Etheredge, of Furnival's Inn, in the county of Middlesex, gentleman, "improvements in the manufacture of bricks, tiles, and tubes."—27th May 1844.

23. To William Basford, of Burslem, in the county of Stafford, brick and tile manufacturer, "certain improvements in the mode of manufacturing bricks, tiles, quarries, and certain other articles made or composed of clay and brick-earth, and of burning and firing the same, and certain articles of pottery and earthenware."—27th May 1844.

24. To William Johnson, of Richmond Hill, in the county of Surrey, Esquire, "certain improvements in machinery for boring, cleaving, cutting, and dressing stone and slate of such kinds as are or may be used for building and for ornamental purposes, and for paving of public and private ways."—28th May 1844.

25. To John Taylor, of Duke Street, Adelphi, in the county of Middlesex, gentleman, being a communication from abroad, "certain new mechanical combinations, by means of which economy of power and of fuel are obtained in the use of the steam-engine."—29th May 1844.

26. To William Walker junior, of Brown Street, Manchester, hydraulic-engineer, "improvements in warming and ventilating apartments and buildings."—29th May 1844.

27. To James Fenton, of Manchester, in the county of Lancaster, engineer, being a communication from abroad, "an improved combination or alloy of metals, applicable to various purposes for which brass and copper are usually employed in the construction of machinery."—31st May 1844.

28. To Joseph Cowan, of Blaydon Burn, near Newcastle-upon-Tyne, merchant, "certain improvements in making retorts for generating gas for illumination."—5th June 1844.

29. To Joshua Procter Westhead, of Manchester, in the county of Lancaster, cotton-spinner, "a new and improved fabric, or new and improved fabrics, and also certain modifications of machinery for making the same, which modifications of machinery are applicable to the manufacture of woven fabrics."—6th June 1844.

30. To George Wilton Turner, of Gateshead, in the county of Durham, doctor in philosophy, "the manufacturing of salts of ammonia and compounds of cyanogen from a substance never before applied to that purpose."—10th June 1844.

31. To Robert Rettie, of Gourock, near Greenock, in the county of Renfrew, in the kingdom of Scotland, civil-engineer, "improvements in gridirons, frying-pans, and other cooking utensils and heating apparatus."—13th June 1844.

32. To James Kennedy, of the firm of Bury, Curtis, & Kennedy, of Liverpool, in the county of Lancaster, engineer, and Thomas Vernon, of the same place, iron-shipbuilder, "certain improvements in the building or construction of iron and other vessels for navigation on water."—24th June 1844.

33. To Charles William Graham, of Kings'-Arms Yard, in the city of London, merchant, being a communication from abroad, "improvements in manufacturing pathological, anatomical, zoological, geological, botanical, and mineralogical representations in relief, and in arranging them for use."—24th June 1844.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Memoir of the late D. F. Gregory, M.A., Fellow of Trinity College, Cambridge. By R. Leslie Ellis, Esq., Fellow of Trinity College, Cambridge.

The subject of the following memoir died in his thirty-first year. He had, nevertheless, accomplished enough not only to justify high expectations of his future progress in the science to which he had principally devoted himself, but also to entitle his name to a place in some permanent record.

Duncan Farquharson Gregory was born at Edinburgh in April 1813. He was the youngest son of Dr James Gregory, the distinguished professor of Medicine, and was thus of the same family as the two celebrated mathematicians James and David Gregory. The former of these, his direct ancestor, is familiarly remembered as the inventor of the telescope which bears his name; he lived in an age of great mathematicians, and was not unworthy to be their contemporary.

Of the early years of Mr Gregory's life but little need be said. The peculiar bent of his mind towards mathematical speculations does not appear to have been perceived during his childhood; but, in the usual course of education, he shewed much facility in the acquisition of knowledge, a remarkably active and inquiring mind, and a very retentive memory. It may, perhaps, be mentioned here, that his father, whom he lost before he was seven years old, used to predict distinction for him; and was so struck with his accurate information and clear memory, that he had pleasure in conversing with him, as with an equal, on subjects of history and geography. In his case, as in many others, ingenuity in little mechanical contrivances seems to have preceded, and indicated the developement of a taste for abstract science.

Two years of his life were passed at the Edinburgh Academy; when he left it, being considered too young for the University, he went abroad and spent a winter at a private academy in Geneva. Here his talent for mathematics attracted attention; in geometry, as well as in classical learning, he had already made distinguished progress at Edinburgh.

The following winter he attended classes at the University of Edinburgh, and soon became a favourite pupil of Professor Wallace's, under whose tuition he made great advances in the higher parts of mathematics. The Professor formed the highest hopes of Mr Gregory's future eminence: those who long afterwards saw them together in Cambridge, speak with much interest of the delighted pride he shewed in his pupil's success and increasing reputation.

In 1833, Mr Gregory's name was entered at Trinity College in the University of Cambridge, and shortly afterwards he went to reside there. He brought with him a very unusual amount of knowledge on almost all scientific subjects: with Chemistry he was particularly well acquainted, so much so that he had been at Cambridge but a few months when it was proposed to him by one of the most distinguished men in the University to act as assistant to the professor of Chemistry; which for some time he did. Indeed, it is impossible to doubt that, had not other pursuits engaged his attention, he might have achieved a great reputation as a chemist. He was one of the founders of the Chemical Society in Cambridge, and occasionally gave lectures in their rooms.

He had also a very considerable knowledge of botany, and indeed of many subjects which he seemed never to have studied systematically: he possessed in a remarkable degree the power of giving a regular form, and, so to speak, a unity to knowledge acquired in fragments.

All these tastes and habits of thought, Mr Gregory cultivated, to a certain extent, during the first years of his residence in Cambridge, of course in subordination to that which was the end principally in view in his becoming a member of the University, namely, the study of mathematics and natural philosophy.

He became a bachelor of arts in 1837, having taken high mathematical honours: more, however, might, we may believe, have been effected in this respect, had his activity of mind permitted him to devote himself more exclusively to the prescribed course of study.

From henceforth he felt himself more at liberty to follow original speculations, and, not many months after taking his degree, turned his attention to the general theory of the combination of symbols.

It may be well to say a few words of the history of this part of mathematics.

One of the first results of the differential notation of Leibnitz, was the recognition of the analogy of differentials and powers. For instance, it was readily perceived that

$$\frac{d^{m+n}}{dx^{m+n}}y = \frac{d^m}{dx^m}\frac{d^n}{dx^n}y,$$

or supposing the y to be *understood*, that

$$\left(\frac{d}{dx}\right)^{m+n} = \left(\frac{d}{dx}\right)^m \left(\frac{d}{dx}\right)^n$$

just as in ordinary algebra we have a being any quantity,

$$a^{m+n} = a^m a^n.$$

This, and one or two other remarks of the same kind, were sufficient to establish an analogy between $\frac{d}{dx}$ the symbol of differentiation and the ordinary symbols of algebra. And it was not long afterwards remarked that a corresponding analogy existed between the latter class of symbols and that which is peculiar to the calculus of finite differences. It was inferred from hence that theorems proved to be true of combinations of ordinary symbols of quantity, might be applied by analogy to the differential calculus and to that of finite differences. The meaning and interpretation of such theorems would of course be wholly changed by this kind of transfer from one part of mathematics to another, but their form would remain unchanged. By these considerations many theorems were suggested of which it was thought almost impossible to obtain direct demonstrations. In this point of view the subject was developed by Lagrange, who left undemonstrated the results to which he was led, intimating, however, that demonstrations were required. Gradually, however, mathematicians came to perceive that the analogy with which they were dealing involved an essential identity; and thus results, with respect to which, if the expression may be used, it had only been felt that they must be true, were now actually seen to be so. For, if the algebraical theorems by which these results were suggested, were true, *because* the symbols they involve represented quantities, and such operations as may be performed on quantities, then indeed the analogy would be altogether precarious. But if, as is really the case, these theorems are true, in virtue of certain fundamental laws of combination, which hold both for algebraical symbols, and for those peculiar to the higher branches of mathematics, then each algebraical theorem and its analogue constitute, in fact, only one and the same theorem, except *quoad* their distinctive interpretations, and therefore a demonstration of either is in reality a demonstration of both.*

The abstract character of these considerations is doubtless the reason why so long a time elapsed before their truth was distinctly perceived. They would almost seem to require, in order that they may be readily apprehended, a peculiar faculty —a kind of mental *disinvoltura* which is by no means common.

Mr Gregory, however, possessed it in a very remarkable degree. He at once perceived the truth and the importance of the principles of which we have been speaking, and proceeded to apply them with singular facility and fearlessness.

It had occurred to two or three distinguished writers that the analogy, as it was called, of powers, differentials, &c., might be made available in the solution of differential equations, and of equations in finite differences.

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* If, as it has been suggested, the values of certain definite integrals are to be looked upon as merely arithmetical results, then in such cases we are not at liberty to replace the constants involved in the definite integral by symbols of operation. In other cases we are at liberty to do so, and this remarkable application of the principles stated in the text has already led Mr Boole of Lincoln, with whom it seems to have originated, to several curious conclusions.

This idea, however, probably from some degree of doubt as to the legitimacy of the methods which it suggested, had not been fully or clearly developed: it seems to have been chiefly employed as affording a convenient way of expressing solutions already obtained by more familiar considerations.

To this branch of the subject Mr Gregory directed his attention, and from the general views of the laws of combination of symbols already noticed, deduced in a regular and systematic form, methods of solution of a large and important class of differential equations (linear equations with constant coefficients, whether ordinary or partial) of systems of such equations existing simultaneously, of the corresponding classes of equations in finite and mixed differences; and lastly, of many functional equations. The steady and unwavering apprehension of the fundamental principle which pervades all these applications of it, gives them a value, quite independent of that which arises from the facility of the methods of solution which they suggest.

The investigations of which I have endeavoured to illustrate the character and tendency, appeared from time to time in the *Cambridge Mathematical Journal.*

In this periodical publication Mr Gregory took much interest. He had been active in establishing it, and continued to be its editor, except for a short interval, from the time of its first appearance in the autumn of 1837, until a few months before his death. For this occupation he was for many reasons well qualified; his acquaintance with mathematical literature was very extensive, while his interest in all subjects connected with it was not only very strong, but also singularly free from the least tinge of jealous or personal feeling. That which another had done or was about to do, seemed to give him as much pleasure as if he himself had been the author of it, and this even when it related to some subject which his own researches might seem to have appropriated.

This trait, as the recollections of those who knew him best will bear me witness, was intimately connected with his whole character, which was in truth an illustration of the remark of a French writer, that to be free from envy is the surest indication of a fine nature.

To the *Cambridge Mathematical Journal*, Mr Gregory contributed many papers beside those which relate to the researches already noticed. In some of these he developed certain particular applications of the principles he had laid down in an Essay on the Foundations of Algebra, presented to the Royal Society of Edinburgh in 1838, and printed in the fourteenth volume of their Transactions. I may particularly mention a paper on the curious question of the logarithms of negative quantities, a question which, it is well known, has often been discussed among mathematicians, and which even now does not appear to be entirely settled.

In 1840, Mr Gregory was elected fellow of Trinity College; in the following year he became master of arts, and was appointed to the office of moderator, that is, of principal mathematical examiner. His discharge of the duties of this office (which is looked upon as one of the most honourable of those which are accessible to the younger members of the University) was distinguished by great good sense and discretion.

In the close of the year 1841, Mr Gregory produced his "Collection of Examples of the Processes of the Differential and Integral Calculus;" a work which required, and which manifests much research, and an extensive acquaintance with mathematical writings. He had at first only wished to superintend the publication of a second edition of the work with a similar title, which appeared more than twenty-five years since, and of which Messrs Herschel, Peacock, and Babbage, were the authors. Difficulties, however, arose, which prevented the fulfilment of this wish, and it is not perhaps to be regretted that Mr Gregory was thus led to undertake a more original design. It is well known that the earlier work exercised a great and beneficial influence on the studies of the University, nor was it in any way unworthy of the reputation of its authors. The original matter contributed by Sir John Herschel is especially valuable. Nevertheless, the progress which mathematical science has since made, rendered it desirable that another work of the same kind should be produced, in which the more recent improvements of the calculus might be embodied.

Since the beginning of the century, the general aspect of mathematics has greatly changed. A different class of problems from that which chiefly engaged the attention of the great writers of the last age has arisen, and the new requirements of natural philosophy have greatly influenced the progress of pure analysis. The mathematical theories of heat, light, electricity, and magnetism, may be fairly regarded as the achievement of the last fifty years. And in this class of researches an idea is prominent, which comparatively occurs but seldom in purely dynamical enquiries. This is the idea of discontinuity. Thus, for instance, in the theory of heat, the conditions relating to the surface of the body whose variations of temperature we are considering, form an essential and peculiar element of the problem; their peculiarity arises from the discontinuity of the transition from the temperature of the body to that of the space in which it is placed. Similarly, in the undulatory theory of light, there is much difficulty in determining the conditions which belong to the bounding surfaces of any portion of ether; and although this difficulty has, in the ordinary applications of the theory, been avoided by the introduction of proximate principles, it cannot be said to have been got rid of.

The power, therefore, of symbolizing discontinuity, if such an expression may be permitted, is essential to the progress of the more recent applications of mathematics to natural philosophy, and it is well known that this power is intimately connected with the theory of definite integrals. Hence the principal importance of this theory, which was altogether passed over in the earlier collection of examples.

Mr Gregory devoted to it a chapter of his work, and noticed particularly some of the more remarkable applications of definite integrals to the expression of the solutions of partial differential equations. It is not improbable that in another edition he would have developed this subject at somewhat greater length. He had long been an admirer of Fourier's great work on heat, to which this part of mathematics owes so much; and once, while turning over its pages, remarked to the writer,—"All these things seem to me to be a kind of mathematical paradise."

In 1841, the mathematical Professorship at Toronto was offered to Mr Gregory: this, however, circumstances induced him to decline. Some years previously he had been a candidate for the Mathematical Chair at Edinburgh.

His year of office as moderator ended in October 1842. In the University examination for mathematical honours in the following January, he, however, in accordance with the usual routine, took a share, with the title of examiner,—a position little less important, and very nearly as laborious, as that of moderator. Besides these engagements in the University, he had been for two or three years actively employed in lecturing and examining in the College of which he was a fellow. In the fulfilment of these duties, he shewed an earnest and constant desire for the improvement of his pupils, and his own love of science tended to diffuse a taste for it among the better order of students. He had for some time meditated a work on Finite Differences, and had commenced a treatise on Solid Geometry, which, unhappily, he did not live to complete. In the midst of these various occupations, he felt the earliest approaches of the malady which terminated his life.

The first attack of illness occurred towards the close of 1842. It was succeeded by others, and in the spring of 1843, he left Cambridge never to return again. He had just before taken part in a college examination, and, notwithstanding severe suffering, had gone through the irksome labour of examining with patient energy and undiminished interest.

Many months followed of almost constant pain. Whenever an interval of tolerable ease occurred, he continued to interest himself in the pursuits to which he had been so long devoted; he went on with the work on geometry, and, but a little while before his death, commenced a paper on the analogy of differential equations and those in finite differences. This analogy it is known that he had developed to a great length; unfortunately, only a portion of his views on the subject can now be ascertained.

At length, on the 23d February 1844, after sufferings, on which, notwithstanding the admirable patience with which they were borne, it would be painful to dwell, his illness terminated in death. He had been for a short time aware that the end was at hand, and, with an unclouded mind, he prepared himself calmly and humbly for the great change; receiving and giving comfort and support from the thankful hope that the close of his suffering life here, was to be the beginning of an endless existence of rest and happiness in another world. He retained to the last, when he knew that his own connection with earthly things was soon to cease, the unselfish interest which he had ever felt in the pursuits and happiness of those he loved.

A few words may be allowed about a character where rare and sterling qualities were combined. His upright, sincere, and honourable nature secured to him general respect. By his intimate friends, he was admired for the extent and variety of his information, always communicated readily, but without a thought of display,—for his refinement and delicacy of taste and feeling,—for his conversational powers and playful wit; and he was beloved by them for his generous, amiable disposition, his active and disinterested kindness, and steady affection. And in this manner his high-toned character acquired a moral influence over his contemporaries and juniors, in a degree remarkable in one so early removed.

To this brief history, little more is to be added; for though it is impossible not to indulge in speculations as to all that Mr Gregory might have done in the cause of science and for his own reputation, had his life been prolonged, yet such speculations are necessarily too vague to find a place here; and even were it not so, it would perhaps be unwise to enter on a subject so full of sources of unavailing regret.

Sixth Letter on Glaciers. Addressed to the Right. Hon. EARL CATHCART.

(Communicated by Professor Forbes.)

ROME, *Feb.* 5. 1844.

MY LORD,—In a letter which I addressed to you on the 29th ult., I gave some account of the few new observations which untoward circumstances permitted me to make, last autumn, upon the glaciers of Switzerland and Savoy. I have, however, had leisure to reflect maturely upon the theory of glaciers, which I have been occupied for two years in endeavouring to mature; and, without pretending to find in it a complete solution of every problem which might be proposed respecting these wonderful bodies, I am perfectly satisfied that it is fundamentally conformable to the laws by which they are governed. Some new analogies, to which your Lordship has referred in your last letter, such as that between glaciers and lava streams, may serve to render the subject more popularly intelligible; and in explaining them, I may have an opportunity of removing, in some degree, the difficulties which have arisen in the minds of candid and intelligent persons, who have studied this theory for the first time—difficulties which would probably disappear of themselves by a more prolonged attention.

I have not had the advantage of seeing the eruption of Etna, to which your Lordship alludes, which was indeed over before I arrived at Naples, and of which I did not even hear for a considerable time after; so small is the sensation which such events excite in the country. I have, however, had an opportunity—probably not less favourable, though far less imposing—of studying the mechanism of plastic lava, in the small currents which, during the months of November and December, were very frequently flowing from mouths *within* the crater of Vesuvius. On the 30th November, in particular, I descended to the bottom of the crater, in order to examine a current of very liquid lava, fifteen or twenty feet wide, which issued from a cavity near the foot of the small cone which occupied the centre of the crater, and from whose top (in the shape of an inverted funnel, or of a blast furnace) there issued smoke and flames,* occasionally accompanied by a discharge of volcanic projectiles. The lava issued in a very steady rapid stream, and spread itself over a gentle declivity with a velocity of not less, I think, than a foot per second.

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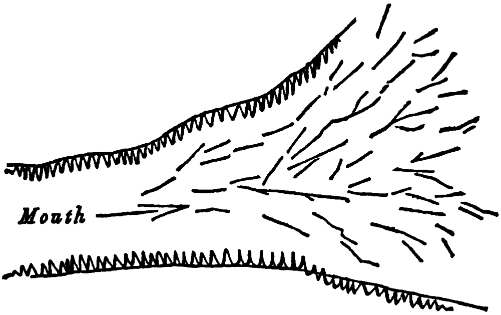
* I am able to add my distinct testimony to that of M. Pilla, as to the emission of *flames* by the crater of Vesuvius. I spent part of the evening of the 1st January on the top, and had not the least doubt that what I saw were actual flames, which issued from time to time from the orifices of the small cone, and which were of a pale colour, often inclining to blue.

Admitting the plastic or viscous theory of glaciers, the resemblance to lava fails (1.), In respect of the great liquidity of the lava near its source; (2.), From its very unequal rate of consolidation; a crust being very soon formed upon the surface, which becoming more and more massive, the principle of fluidity is not uniformly distributed throughout the mass, as in the glacier, but a tolerably perfect fluid struggles with the increasing load of its ponderous crust, which it tears and rends by the mighty energy of hydrostatic pressure; and here and there finding a freer exit far removed from its source, tosses high those mighty fragments of the stony arch which confined it into the wild shapes which strike the eye in crossing the wastes of a lava stream, and which seem at first incompatible with the fluid or semifluid principle of motion. This second circumstance, then,—the very unequal and rapid superficial consolidation of the lava near its source,—has no analogy in a glacier, nor even in a river, unless when breaking up a ponderous crust of ice after a sudden thaw. The regulated progression of the glacier, swiftest in its centre, and with a graduated retardation towards the sides, has a much more precise analogy to that of a river than the lava stream has, which is subdivided (when it has any considerable breadth) into many little currents, each rolling past, and being retarded by its more sluggish or already consolidated neighbour; so that its surface resembles that of the bed of many torrents in the Alps, where the more solid matters, the rocks, stones, gravel, sand, and clay, trace out the form of a sluggish mass propelled downwards by gravity, whilst its surface is seamed by the trickling of innumerable rills of water, charged with the more portable materials which have been washed down, or squeezed from the general mass.

There are other circumstances, however, in which the analogy of the glacier with the lava stream is more complete; and of these I shall observe—

I. That the *cracks* of the dark-coloured slag on the surface of the liquid lava, as it spreads itself abroad, on issuing from the fiery mouth, are *radiated* exactly as those of a glacier under similar circumstances, and which I have represented in the margin as I saw them on Vesuvius, the lines of fissure being marked by the liquid fire shining through. A perfect analogy here exists with the phenomena of radiating fissures in ice, which I first described in the glacier of the Rhone, and afterwards in the ice of the Gl. du Talefre, where it joins the Gl. de Lechaud, in the Gl. of Arolla, and very many other instances.



Fissures in the Crust of Lava during Crystallization.

II. That the slags, where solidified, presented *striæ* or *ripple-marks* along their surface, parallel to the direction of the "ribboned structure" of glacier ice, *i. e.*, inclining slightly from the sides towards the centre of the current, in the direction in which the current is moving. These striæ, or ripple-marks, which have a striking analogy in certain cases of the retarded movement of rivers, are carefully to be distinguished, on the one hand, from the *cracks or flaws*, and, on the other, from the *direction of motion* of the fluid particles.*

III. When, at some distance from the source, the lava became viscid and tenacious, and forced itself, in streamlets of a pasty consistence, through the interstices of its slag, thence it became streaky and drawn out, in the direction last mentioned, as molten glass does in the hands of the workman.

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* A long accidental delay in the printing of this letter enables me to add, that I have found in the lavas of Etna a yet far more perfect analogy to the veined structure of glaciers than that described in the text. It is, indeed, so completely developed as to leave no doubt as to the identity of origin. Aug. 1844.

IV. But there is a more striking analogy to the ribboned structure of glacier ice, to be found in lava currents at a distance from their origin, and where by any circumstance their surface has been broken up, and their internal structure exposed. In the Fossa della Vetrana, for instance, and other places, I have found the lava divided into thin layers parallel to the interior of the surface of the channel through which it flowed, evidently produced by the adhesion or retardation which the soil exerted upon its adjoining film of lava, and the successive portions of lava upon one another, in proportion as the semifluid mass, rolling upon its own particles (or rather sliding imperfectly over them), produced a solution of continuity and a series of shells, parallel in direction to the bed upon which the whole rests. The thickness of these shells varies from one-third of an inch upwards. I have never, however, observed a structure in the *interior* of the lava except that parallel to the sides and bottom of the canal in which it moves; nothing, in short, corresponding to the *frontal dip* in glaciers. But this is quite natural and conformable to the very different constitution of a glacier; and, in particular, it corresponds to the fact so often urged as a difficulty to the semifluid theory of glaciers, namely, the want of ductility or tenacity of their parts. It is that fragility precisely, which, yielding to the hydrostatic pressure of the unfrozen water contained in the countless capillaries of the glacier, produces the *crushing action* which shoves the ice over its neighbour particles and leaves a *bruise*, within which the infiltrated water finally freezes and forms a blue vein. In the lava, on the other hand, where the tenacity is great, the discontinuity, if produced at all, is soldered up by the plasticity of the parts, whose small crystalline structure farther tends to obliterate the separation. The layers just mentioned, parallel to the bed, are perhaps produced by the successive adhesion of warmer streams of lava to the colder parts already deposited, and, consequently, their analogy to the glacier structure must not be pushed farther than as shewing the directions of the tendency to separation of a very viscid stream, powerfully retarded by its bed. It is the congealing of the lava which makes its adhesion to the sides great enough, and its own fluidity small enough, to bear a comparison with the far less ductile body of a glacier. In the heart of the mass where the same intestinal motions take place (as I have shewn conclusively by using *coloured* layers of plastic matter in the models formerly exhibited to the Royal Society), the displaced particles reunite and consolidate into a homogeneous mass without any trace of dislocation.*

V. The convexity or concavity of a semifluid stream like a current of lava or of a glacier, depends entirely upon the relations or conditions in which it is placed. Upon the same slope, a fluid of one degree of consistence will run off in a concave stream, whilst a more viscid one, which must accumulate in thickness, in order to overcome the resistance in front (just as water which meets a sudden obstacle), rises into a convex curve. This is perfectly seen in the case of a substance like plaster of Paris, mingled with water, whose consistence may be varied at pleasure, and a stream of which may be made either concave or convex, or concave at its origin and convex at its termination, as is the case with a glacier. The evidence on this subject, afforded by the models formerly laid before the Royal Society, is so complete and conclusive, that, however interesting it might be to put into a mathematical form the relations of the *constants* of the effect of gravity, the viscosity of the body, and the retardation of the sides, as affecting the form of the surface, it is sufficient for my present purpose to appeal to facts so familiar, and experiments so easy, that their evidence may well be preferred to the more casual and embarrassed case of lava streams, which, as I have already observed, are seldom or never to be regarded, on a great scale, as *simple* moving masses. I may, however, add, that when the inclination is small the surface is convex, at a certain distance from the origin.

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* The following passage from M. Dufrenoy's Account of Vesuvius, is interesting, if it were only as recording his remark, that the variation of velocity in different parts of a stream must produce longitudinal striæ. "La plupart des coulées presentent des bandes longitudinales assez paralléles entre elles ; ces larges striés saillantes sur la surface sont les traces du mouvement de la lave qui ne s'avance pas d'une seule piece, mais par bandes paralléles." Sur les Environs de Naples, p. 324.

VI. There is a circumstance attendant on the motion of lava streams, which has struck several geologists, before the viscous theory of glaciers had been proposed—I mean the existence of *moraines*. The moraines of lava are best seen in the more defined and united lava streams on rather a small scale,—those, in short, which have the unity and character of a proper stream, moving at once in its various parts. The moraine is composed of stranded masses of lava crust, thrown aside by the liquid fiery stream, and partly, perhaps, of the yielding matter of the bed of the stream pressed outwards and upwards by the hydrostatic pressure of the centre. The former is chiefly, perhaps, the case when streams of tolerably fluid lava flow down a steep inclination, as on the exterior of the cone of Vesuvius; the latter, when the inclination is small and the weight of accumulated lava great. The igneous moraines, though noticed by various geologists, are most emphatically described by M. Elie de Beaumont, in his masterly memoir on Etna, in the following words :—" Une des circonstances que les coulées de lave présentent le plus invariablement toutes les fois qu'elles ont parcouru des talus où elles pouvaient acquérir une certaine vitesse, caractères que j'ai observés sur toutes sortes de pentes depuis 33° jusqu'à 2° et que je n'ai cessé d'observer que là où les coulées se sont arrêtées faute de pente, consiste en ce que chaque coulée est flanquée de part et d'autre par une digue de scories accumulées qui rappelle par sa forme la moraine d' un glacier ; digue qui s'élève constamment à une hauteur supérieure à celle à laquelle la coulée est reduite à la fin du mouvement, et qui marque le maximum de hauteur qu'elle a atteint dans le moment de son plus grand gonflement. Souvent aussi les coulées présentent de pareilles digues vers leur milieu, lorsqu'elles sont partagées en plusieurs courants distincts coulant l'un à côte de l'autre."*

VII. The termination of a lava stream on a level or slightly inclined surface due to its increasing viscidity, presents appearances almost identical with those of a glacier. The same protuberant convexity of surface, the same steeply-inclined sides and front, and nearly the same ground-plan, all bespeak a similarity in the circumstances of motion. I may add, that in some experiments which I made some years ago upon the flowing of melted iron in narrow channels, and upon small slopes, with a view to illustrate some phenomena of lava streams, before I had commenced a particular study of glaciers, I arrived at similar results, and obtained the same convexity of surface which is produced in the plaster models before cited.

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* E. de Beaumont Recherches sur le Mont Etna, p. 184.

It is very interesting to observe how many intelligent persons have been struck with the similarity between glaciers and lava streams, without, however, pushing the parallel beyond a general resemblance. M. Elie de Beaumont, we have seen, speaks of the moraines of volcanoes; but in various parts of his writings, as well as those of his colleague, M. Dufrenoy, we find the mention of glaciers as continually suggested to his mind when surveying the wastes of Etna and Vesuvius. One of these passages is the following: "L'ecorce superieure d'une coulée separée de l'ecorce inferieure et du sol sousjacent par une certaine epaisseur de lave liquide ou du moins visqueuse, se trouve dans un état comparable à celui d'un glacier, qui, ne pouvant adhérer au sol sousjacent à cause de la fusion continuelle de sa couche inférieure, se trouve contraint à glisser;"* shewing that the author then adopted the theory of Saussure (since ably defended by Mr Hopkins), in which the fusion of the ice by the heat of the earth, might be said, in some sense, to *float* down the superincumbent solid; an opinion best controverted by the fact which M. E. de Beaumont has since clearly brought into notice, that under existing circumstances such fusion is perfectly insignificant.†

The writer of a popular Italian guide-book, Mrs Starke, is perhaps one of the first who indicated the striking general resemblance of a stream of lava to a glacier. She describes the former (which she saw during a small eruption of Vesuvius) as "rolling, wave after wave, slowly down the mountain with the same noise, (?) and in the same manner, as the melting glaciers roll into the valley of Chamouni; indeed, this awful and extraordinary scene would have brought to mind the base of the Montanvert, had it not been for the crimson glare and excessive heat of the surrounding scoriæ."*

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* Recherches sur l'Etna, p. 177.

† Annales des Sciences Géologiques par Rivière.

Mr Auldjo, the author of a Narrative of an Ascent of Mount Blanc, and therefore acquainted with the appearance of glaciers, has renewed Mrs Starke's comparison in very similar expressions, in a work more recently published upon Mount Vesuvius. Captain Basil Hall has, if I mistake not, in more than one part of his writings suggested the picturesque analogy of volcanoes and icy mountains, the cradle of glaciers.

We have seen how far there is a real analogy between the mechanism of these two terrible scourges of Almighty power —the ice-flood and the fire-flood, both of which invade the homes and the labours of man, with a force alike irresistible. But to render the analogy more than apparent or poetical, it was required that several difficulties, very obvious, and seemingly insuperable, should be removed; and the chief of these was the texture of ice compared to the texture of lava—the former passing from a brittle solid into limpid fluid by heat, the latter passing like sealing-wax through every intermediate degree of viscidity. This difficulty could only be met by an exact determination of the question—Of how far a glacier is to be regarded as a plastic mass? Were a glacier composed of a solid crystalline cake of ice, fitted or moulded to the mountain bed which it occupies, like a lake tranquilly frozen, it would seem impossible to admit such a flexibility or yielding of parts as should permit any comparison to a fluid or semifluid body, transmitting pressure horizontally, and whose parts might change their mutual position, so that one part should be pushed out whilst another remained behind. But we know, in point of fact, that a glacier is a body very differently constituted. It is clearly proved by the experiments of Agassiz and others, that the glacier is not a mass of ice, but of ice and water; the latter percolating freely through the crevices of the former, to all depths of the glacier; and as it is matter of ocular demonstration that these crevices, though very minute, communicate freely with one another to great distances, the water with which they are filled communicates force also to great distances, and exercises a tremendous hydrostatic pressure to move onwards in the direction in which gravity urges it, the vast, porous, crackling mass of seemingly rigid ice, in which it is, as it were, bound up.

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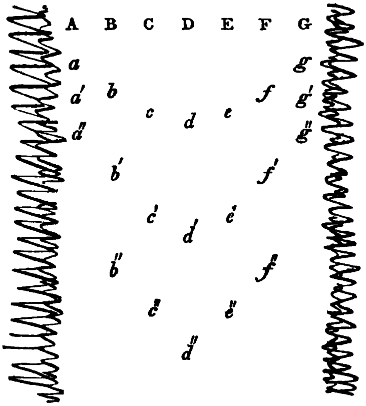
* Starke's Travels. French edit., p. 311.

But farther than this, the experiments first announced in the earliest of these letters, shewed, that whatever be the constitution of a glacier, and whatever be the cause of its motion, THE FACT IS, that it does not move like a solid body sliding down a bed or channel, but that the velocity of each part of its breadth is different. It was demonstrated by the most clear and plain geometrical measurements, that whilst the centre of a glacier moves 500 feet, the side of the glacier moves only 300; consequently, the portions of ice which started together soon part company, and the central molecule has completed its course, or arrived in the lower valley, whilst the other, which was its companion, has advanced only three-fifths of the distance, or remains perhaps several miles behind. Thus it has been shewn from multiplied measurements of the most precise and accordant kind, that a series of stones or marks being supposed to be laid across a glacier in the line ABCDEFG; they will be found, after a certain time, in the



position $abcdefg$, after other equal intervals at $a'b'c'd'e'f'g'$, and at $a''b''c''d''e''f''g''$, by which time it will be seen that the neighbour particles have entirely changed their relative positions, and that the mass can have no pretension to be called rigid, but moulds itself after the manner that a fluid or semifluid body does in like circumstances, the centre advancing fastest, and, for some space in the centre, nearly uniformly, whilst the retardation produced by the friction of the banks is most intense in their neighbourhood; which is conformable to what we know of the movement of viscous fluids. It is, therefore, no hypothesis, but a simple statement of a demonstrated fact, *that the manner of movement of the surface of a glacier is not such as is consistent with the continuity of a rigid body, but that it coincides with the manner of motion of a viscous or semifluid body.* Whatever may be the difficulty of conceiving the glacier to be a body thus constituted, the fact admits of no doubt;—the effects of forces applied on a great scale to bodies, are the best and only conclusive proofs of their real constitution, and worth all molecular theories and minute experiments put together.

If a body be really of a *pasty* consistence, ductile and plastic like lava or tar, such transpositions taking place in the interior of the mass are effected without any injury to the texture or continuity of the substance. With a degree less of plasticity, a violent separation of the parts may take place, but they will, by juxtaposition, soon reunite and take a new *set*. With a degree more of rigidity, there must be a permanent *bruising* and *rending* of the parts, in order that a semi-rigid body may assimilate all in its movements to a fluid. It must, therefore, be considered as entirely confirmatory and explanatory of the preceding statements of the seeming plasticity of a body so fragile in its elements as pure ice, that the ice of glaciers is found rent in many parts by the forces tending to dislocation, and that, besides, it contains within itself a testimony to the internal partial movements by which its total motion is effected, in the veined structure already alluded to, occasioned by the varying velocity of the adjacent icy strips A a a' a'', B b b' b'', &c. This structure is not exactly parallel to the direction of motion of the ice, for reasons which I have elsewhere stated, but which need not now be adverted to. My present object is to shew, that the rigidity of ice, as a physical fact, cannot contradict the mathematical evidence of the manner in which glaciers *do* move, and that the seeming contradiction is reconciled by shewing, that the ice bears permanent traces of the violent strain to which it is subjected, and of the actual bruising and disseverment of its parts, producing a phenomenon otherwise impossible to be explained.

I believe that it is during the progress of the glacier thus subjected to a new and peculiar set of forces depending upon gravity, and which remodel its internal constitution, by substituting hard blue ice, in the form of veins, for its previous snowy texture, that the horizontal stratification observed in the higher part of the glacier or *névé*, is gradually obliterated.

If, as we cannot doubt, the slower motion of the glacier near its sides be owing to the retardation which their excessive friction occasions, there must necessarily be a retardation at the bottom in a similar manner, and the surface of the glacier will move faster than the strata in contact with the ground; to which it is even supposable, that, in some cases, they may be entirely frozen. This retardation may, perhaps, be less than the lateral retardation, because the slope of the valley in which the glacier lies is probably more even, generally speaking, than its breadth is regular. In fact, so great is the irregularity of the ground-plan of any compound valley, —so frequent the interfering ridges or promontories, the bays formed by adjoining tributary valleys,—and so numerous the gorges or contractions,—that we cannot so properly call the lateral resistance to the onward motion of a glacier, *friction*, but rather a direct opposition to the exit of a solid body, which renders its plasticity absolutely essential to its progression. Nevertheless, the inferior slope of the glacier bed being also irregular, and its friction great, must cause a retardation in the lower strata of ice, which must be continually overtaken by the superior ones: and this appears to me to be so plain and necessary a consequence of the combination of facts which we have to consider, that perhaps the direct proof of it would not repay the labour which it would involve, which would be of the most serious kind;—for we must not expect to find the difference of velocity apparent in the superficial strata, even to a considerable depth, since we know that the retardation is a maximum near the sides and bottom, and that, for the same reason, the motion of all the central part of a glacier is nearly uniform, so will the motion of all the part of the ice near the surface be nearly uniform.

These considerations suggest the explanation of a difficulty, kindly suggested to me by a most competent judge, who expressed himself at the same time persuaded of the truth of the viscous theory of glaciers. "How comes it, that, if the motion of the different parts of a glacier diminishes from the surface to the bottom, the 'trou de sonde' or *bore*, 140 feet deep, made by M. Agassiz in the glacier of the Aar, is stated to have remained vertical for a period of many weeks?" In the first place, the *fact* of the verticality requires confirmation; for it is difficult to understand how, by means of a plummet, a hole 140 feet deep, and only 3 or 4 inches in diameter, could have its verticality tested. Such bores, so far as I have seen them, are more or less twisted, owing to the softness of the material, and the method of working; and it seems beyond all probability, that a hole of such a depth constructed in the ordinary way, should be either mathematically straight or vertical. I apprehend that the verticality alluded to by M. Agassiz, or his coadjutors, is merely that of popular language, indicated by the boring rods standing vertically outwards when plunged into the hole, which, on account of their flexibility, would not be an indication of the verticality of more than the upper twenty or thirty feet of the bore at the most.*

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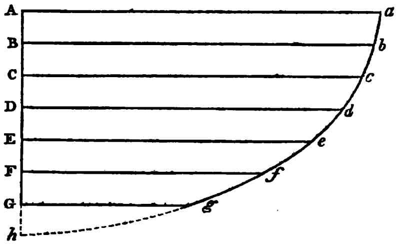
* Since this passage was written, I have had an opportunity of referring to the description of the experiments of Agassiz in the *Bibliothèque Universelle;* and I find that there is no evidence whatever of the continued verticality of the bore of 140 feet, which existed (to that depth), I believe, but a few days: the observations of continued verticality, such as they are, applied to small bores only, not exceeding 25 or 30 feet, which, of course, greatly increases the force of the reasoning in the text. Aug. 1844.

But, even setting aside this important consideration, the principle of the variation of velocity being chiefly confined to the neighbourhood of the sides and bottom, and the comparatively quiescent and passive state of the central and superficial part, seem sufficient to explain the facts within the reasonable limits of error. The depth of 140 feet appears, from M. Agassiz's own observations, not to exceed ONE-SIXTH, at most, of the depth of the glacier of the Aar in that part. Now, let A B C, &c., represent points in the vertical section of the glacier; then, from all that we know of the superficial motion of glaciers, or of the parallel case of rivers whose velocity has been ascertained at different depths, the velocities will vary in some such manner as A *a*, B *b*, C *c*, &c.,—the variation being scarcely sensible at first, and very rapid at the bottom, where the velocity may even be zero, if the curve be prolonged to the point *h*. But, supposing G to be the bottom of the glacier, it will be seen how insignificant may be expected to be the variation of velocity between A, the surface, and B, one-sixth of the depth, during the short period of a few weeks, or even months. I have the honour to be, &c.



Seventh Letter on Glaciers.—On the Veined Structure of the Ice. Addressed to the Rev. Dr WHEWELL, Master of Trinity College, Cambridge. By Professor FORBES.

SALERNO, *May* 18. 1844.

* * * * * * * *

You object that the shells produced by the rupture of the parts of the ice caused by excessive friction, should be all parallel to the sides and bottom of the trough of the glacier, instead of being inclined from the sides inwards and forwards towards the centre, as in Fig. 1,

Fig. 1.

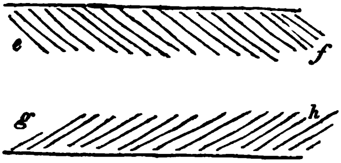
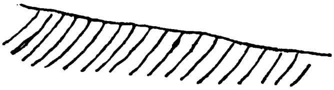


Fig. 2.

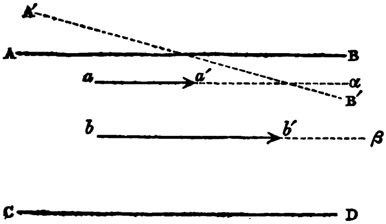


and from the bottom upwards and forwards, as in Fig. 2. You will find that I have endeavoured to explain this in the last chapter of my book of Travels; but not having it by me, I cannot refer you to the particular passages. The point in question is undoubtedly the least obvious and most difficult part of the theory; but as I have no doubt of its exactness, it will have a proportionate weight in deciding in its favour the opinion of persons accustomed to mechanical theories. It would be difficult to bring it home to the apprehension of ordinary readers; and, for this reason, I have dwelt upon it, perhaps, too shortly in the chapter alluded to.

You will readily admit, that if I shall demonstrate separate reasons for the existence of each of the structures figured above, (the first a plan, the second a section), the result will be the spoon-shaped structure which I have shewn to exist in glaciers.

(1.) The tearing asunder of the particles of the glacier, owing to the friction of the sides is, *nearly*, but *not quite*, parallel to the sides; for this reason, that the lines of greatest strain are determined, not merely by the force of gravitation which urges the particles forwards, but there is a *drag* towards the centre of the stream, in consequence of the greater velocity there.

Fig. 3.



Let A B be the side of the glacier, whilst the particle *a* moves to *a′*, the central particle *b* moves to *b′*, which, owing to the cohesive bond between *a* and *b* must produce a strain oblique to the axis of the glacier.

Or view the matter thus—the movement of the ice stream (considered just now solely as respects its surface), is effected against a varying resistance. The line of particles in the direction $a\,\alpha$ presents a greater force of opposition to the movement of the particle a, than the line of particles $b\,\beta$ presents to the movement of b. This is owing to the lateral friction acting more powerfully in retarding the first than the second; consequently the *virtual* wall of the glacier, or plane of complete resistance, will be no longer A B, but inclined (for the particle a) in the direction A′ B′.

If this reasoning require support from experiment, it is easily had. I have described, in a foot-note to my last chapter, the experiment of dusting powder upon a moving viscous stream; and our friend Heath has now a specimen of the result, shewing the lines of separation in the direction I have stated. The same is remarkably shewn in the case of a stream of water; for instance, a mill-race. Although the movement of the water, as shewn by floating bodies, is exceedingly nearly (for small velocities, sensibly) parallel to the sides; yet the variation of speed from the side to the centre of the stream occasions a *ripple* or molecular discontinuity, which inclines forward from the sides to the centre of the stream at an angle with the axis, depending on the ratio of the central and lateral velocities. The veined structure of the ice corresponds to the ripple of the water, a molecular discontinuity whose measure is not comparable to the actual velocity of the ice; and, therefore, the general movement of the glacier, as indicated by the moraines, remains sensibly parallel to the sides.*

(2.) If I have explained myself distinctly as respects the fissures produced by lateral friction, there will be little difficulty in applying the same reasoning to the resistance of the frontal dip, exhibited in the second figure of this letter. When a fluid, or semi-fluid, is very viscous, there is a great resistance to its onward motion in the direction which gravity and the fall of the bed prescribes. Let L M be the surface, N O the bed of a glacier; then the resolved force is usually considered as acting on the particles *m n*, in the directions *m m′*, *n n′*, parallel to the bed. But if we reflect that, owing to the length of the glacier, and the toughness or consistency in its mass, the resistance of the line of particles *n v* is enormous, the plane of complete resistance N O will virtually be twisted in the direction N′ O′, and the particle tends to be thrust *forwards* and *upwards*, which will evidently produce the frontal dip.

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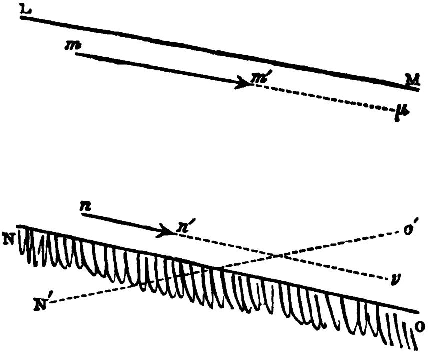
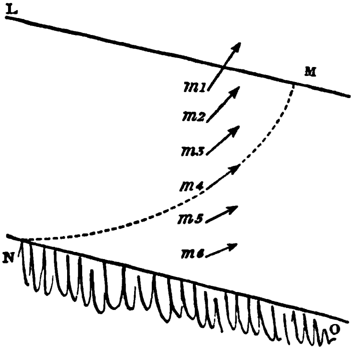
* have lately identified completely the planes of separation in the lava streams of Etna, which correspond perfectly to those of the glacier, being nearly vertical at the sides, and directed slightly towards the centre of the stream.

Fig. 4.



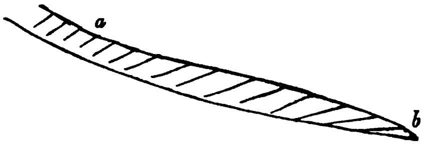
(3.) But there is a peculiarity in the vertical plane which did not exist in the horizontal one. In the case we first considered, the veined structure exists almost entirely in the neighbourhood of the sides of the glacier, and is lost towards its centre, being due to the influence of friction, which varies with the distance from the side; the central part, *e f g h* (Fig. 1.), moving nearly uniformly, would cease to exhibit a linear arrangement. The completion of the curve is due to the influence of the curvilinear bottom, combined with the opposing mass of the glacier in front; and this influence will extend to the very surface, as a little consideration will shew. For, resuming the construction of Fig. 4, since a vertical series of particles, m_1 . . m_6 (Fig. 5) are supposed to be acted on by a force partaking of the nature of hydrostatic pressure, derived from a great elevation, each particle is ready to move onward in the direction in which the effective pressure is greatest; and it is plain, that, owing to the diminishing relation between the weight of the superincumbent particles and the frontal resistance, the direction in which the particles will tend to slide over one another, or to produce rents, will approach verticality at the surface, and on the whole will, therefore, tend to produce lines of discontinuity, such as N M.

Fig. 5.



(4.) Considering the glacier at different points of its length, it is evident, by similar reasoning, that near the region of the nevé *a* the frontal dip will be all but vertical, because there the horizontal resistance is enormous; whilst at the lower end *b*, where it tends to vanish, the shells will tend to parallelism with the bed. It is needless to add, that the relative movements of the particles over one another, producing discontinuity, are not to be confounded with their absolute motions in the glacier, exactly as under head (1.) I must however, observe, that as the tendency of any particle due to the hydrostatic pressure will be to describe ultimately the whole curve N m_4 M within the glacier, this may account for some of the facts, or supposed facts, which indicate a tendency in the ice to expel bodies engaged in it, as well as the convexity of the glacier at all times, and its remarkable rise of surface during winter.

Fig. 6.



Lastly, The ablation of the surface of the glacier during its descent from *a* to *b*, (Fig. 6.) will tend continually to give the observed elongated forms of the superficial bands, by cutting the shells of structure obliquely.

I remain, my Dear Sir, yours sincerely,

James D. Forbes.

To the Rev. Dr Whewell.

On the Ancient Peruvians. By Dr J. J. de Tschudi.*

Communicated by the Ethnological Society.

During a stay of five years in Peru, spent for the most part in the interior of that remarkable country, I devoted as much of my time as I could spare from my studies in Natural History to the investigation of the condition, past and present, of the aboriginal inhabitants. In the course of these researches I collected many facts connected with their history and manners. I have thoroughly examined more than eighty ruins of Indian villages, with, perhaps, half that number of tombs. I have seen and described many of their relics, and have brought to Europe ten mummies of different ages and sexes (six others are still expected); and more than thirty skulls of Indians are lying before me, the most beautiful collection that has ever been obtained from that part of America.

I shall, probably, at some other time have the pleasure of bringing before the Ethnological Society my researches on the great migrations of the nations of the northern division of the New World, together with my views on the different tribes and races: for the present I shall communicate a few general remarks only.

The greater part of the old Indian villages in the Sierras of Peru, are situated on steril heights, conical turreted hills, summits of mountains or narrow ridges, and on an eastern exposure. The choice of this latter situation was determined by their religion. It was, in fact, natural that the Indians, who considered their kings to be the offspring of the sun, which they adored as their primary divinity, should have chosen, for the sites of their towns and villages, positions from which they could see and adore the god at his first appearance above the horizon. To this practice, which in some provinces was very rigidly followed, they sacrificed much of their comfort, as they were not only exposed to violent and icy winds, but also found themselves on points totally deprived of water, which, in some cases, had therefore to be brought from a distance of two or three miles. This explains why we find in certain ruins of Indian villages, especially such as are situated at a distance from springs or brooks, so great a number of water-pots of all sizes, forms, and materials. In these pots the indispensable fluid was fetched from a distance on the backs of Llamas. I found the same custom still subsisting among the Indians.

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* Read before the Society, 1844.

In all large villages, where the ground permitted, a great central square was formed, from which very regular streets frequently branched off in all directions. The structure of the houses is extremely varied. Close to the largest palaces, having from twenty to twenty-five windows in front, are the smallest, narrowest, and poorest cottages. Stones and cement are almost everywhere the usual building material; but near the coast, on the western side of the Cordilleras, larger edifices of bricks are found, and called by the Indians Ticacuna.

In the districts of Tunin and Ayacucho, I have seen large villages consisting of tower-like buildings of a very peculiar structure. Every house is round or quadrangular, the inner diameter being about 6 feet. The walls are from 18 inches to 2 feet thick, and the height of the whole building seldom exceeds 20 feet. The entrance opens towards the east or the south, and is, at the utmost, 2 feet high. Having crawled in, we find ourselves in a space of about 6 feet across, and of equal height. The walls are rude and bare, but in them are deep holes, which must once have served as cupboards, as we still find in them very frequently maize, corn, small pots, &c. No window enlightens the space. The roofs of these rooms consist of several horizontal immured flagstones, which, in the middle, do not touch each other, but leave an open space about one foot and a half broad. By this opening we may ascend, and arrive, not without difficulty, at the second story, which is built in the same manner, but has generally some openings instead of windows. The roof is the same as the lower one, and through it we come to the upper story, the roof of which forms that of the whole house, and consists of very solid masonry. The upper story is generally lower than the other, and probably served as a store-room. I once, however, found in it the well-preserved mummy of a child. The family lived on the ground-floor. We can distinguish very clearly the place where they used to cook. The one immediately above was the sleeping-room ; a great flagstone is often found in it, which served to cover the opening. The old Indian fortification Hinckay is of entirely similar structure, though on a grander scale. I have felt very comfortable in these small and narrow dwellings ; they frequently protected me for hours from violent rain, after I had expelled a fox or a zorillo from them.

I have often found in these houses the best preserved mummies and other antiquities. Only a small part of the dead were buried in tombs of masonry, in the so-called Huaca, or more correctly Aya-huaci (Dead house). Near the coast the bodies were laid, many together, in certain places in the sand ; in the mountains, however, in caves, in fissures of the rocks, or in their own houses. When the last was the case, I observed the following arrangement. Immediately below the surface, and only covered with a thin stratum of earth, the bodies are placed, more or less preserved, mostly, but not always, in a sitting posture. The head, in this case, is supported by the hands, the elbows by the thighs, and all the fingers of each hand are tied together with a string, which, running across the neck, connects both hands.

If we remove the bodies and clear away the second stratum of earth, we arrive at the domestic implements of the Indians, cooking and water-pots of clay, calabayos, huallcas, implements of war and hunting. Below this stratum there followed the third and last, which contain the gods ; they are mostly made of clay, but sometimes also of silver and gold ; such idols have been found in different places, which contained from twenty-five to thirty pounds of the finest gold.

On the eastern side of the Cordilleras, large huacas are very scarce ; but they are frequently met with in the coast districts of Peru. The mummies deposited in the fissures of rocks cannot often be removed without extraordinary difficulty; and it appears incomprehensible how the dead bodies, with all their muscles attached, could be forced into them. Most curious groups of mummies are found, which strongly excite our curiosity. One of the most interesting was discovered in the fortification Huickay mentioned above:—A woman in the act of delivery, in a sitting posture, presses with her knees forcibly against the back of a man, who is squatting before her, and keeps hold of his shoulders with her hands spasmodically contracted; the head of the child is already born, but the trunk and extremities are still in the genitation of the mother. I intended to have sent this peculiar group to Europe, but in my absence it was destroyed by the brutality of a European. I found another group in which a child kept firmly hold of the nipple of the mother. Together with the mummies are frequently discovered skulls and skeletons of animals, especially of the mammiferous genera, canis,* felis (Felis onca, and concolor), lutra, mephitis, lagidium, anchenia; of birds, condors, owls, ramphastidæ, prittaciæ. With the mummies of children, which I dug out in the Palace of Tarmotambo, I found the specimens of a species of Arara, not natives of Peru, but only of the northern parts of South America. Of reptiles, the tortoise is the only one which was buried with the dead. I have never observed any remains either of Saurians or Ophidians.

Regarding the skulls, I will here only mention one very singular peculiarity. In the children of that part of the primitive inhabitants of Western South America, who were distinguished by a flattened occiput, a bone is found between the two parietal bones, below the lambdoidal suture, separating the latter from the inferior margin of the squamous part of the occiput. This bone is of a triangular shape—its upper angle lies between the ossa parietalia, and its horizontal diameter is twice that of its vertical. It coalesces at very different periods with the occipital bones, sometimes in the first month after birth, and sometimes not until after six or seven years. In one skull belonging to a child about seven years old, with a very flat occiput, this line is separated by the most perfect suture from the squamous part of the occiput, and is 4 inches broad and 2 inches high. In a more advanced age, it probably completely integrates with the rest of the skull. I have, however, perceived it in all the skulls of this class which I have examined. On a close scrutiny, we generally find traces of it in the linea semicircularis superior.

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* I hope to shew in the second number of my Fauna Peruviana, that the dog, Canis familiaris, was indigenous to Peru, and not introduced by the Spaniards.

This bone, which, in remembrance of the nation in which it is found, I call *Os Incæ*, corresponds entirely to the Os interparietalia of the Rodentia and Marsupialia. We know that it exists in these classes of mammalia through life—that it also occurs in the fœtal state of several pachydermata, ruminantia, carnivora, &c. In the ordinary embryos of man, there are barely some traces in the first months, which, however, soon disappear. I think it, therefore, very curious that we should find so retarded a formation in a whole race of men, who have exhibited a very inferior degree of the intellectual faculties.

I have just heard that Mr Bellamy, in a paper on Peruvian Mummies, read before the British Association on the 3d of August 1841, and printed in the Annals and Magazine of Natural History, October 1842, has already pointed out this peculiarity in the osseous structure, and I am much pleased to confirm his observations by the examination of more than a hundred of such skulls.

I may, however, observe, that Mr Bellamy certainly did not obtain his mummy from the high plains of Peru, as in those districts there occurs no drift sand strongly impregnated with salt. In those plains the mummies are not found in any quantity at a short distance below the surface; and, lastly, Captain Banckley, who could obtain any quantity of mummies at Arica, or some other seaport town, would certainly not have taken the trouble of fetching them from the high plains. Dr Bellamy is also too hasty in determining the race of the nation to which these skulls belonged, especially if he ascribe them to that nation, which is said to descend from Asiatics, who emigrated with Manco Capac.

I transmit to the Society the drawing of a skull, which I dug out of the old Indian fortification Thrickay. It belongs to one of the three typical races of the former inhabitants of Peru, and is not to be confounded with those figured by D'Orbigny under the denomination of Aymara.

In the hope of throwing some new light on the question in dispute between Dr de Tschudi and Mr Bellamy, Dr King entered into correspondence with the latter gentleman, which drew forth this reply. "In the very rough communication which I had the honour of making to the British Association, I confined myself as much as possible to facts, just venturing enough, in the way of opinion, to draw on discussion. I am delighted that the time is at length arrived, for something favourable to science must be the result. My knowledge, however, is far too limited to permit of my joining in any argument that may be advanced; all I can do, is to take care that no misstatement is made of what I have made public.

"It has been, and I fear always will be, my misfortune to write from my own fireside, for my avocations have, and probably ever will, keep me at home. I have little or no geographical knowledge of Peru, and of its minute physical characters I know less. Dr de Tschudi, I presume, from the boldness of his assertions, is a traveller, and that he has visited the part of the world in question. Hence, doubtless, he is correct, when he says that the mummy was not brought from the high plains of Peru, for the reasons he gives appear to be too forcible to admit of any doubt. We have, in fact, from him what looks very much like personal observation, for he says, 'in those districts there occurs no drift-sand,' &c.

"Captain Blanckley, from whom the mummies were procured, some little time after they fell under my notice, went abroad, and I have in vain several times since endeavoured to communicate with him. In my paper I have said, after regretting my inability to furnish information of a more correct character, that he 'stated to me in conversation, that he exhumed them himself from an elevated part of land in the mountainous district of Peru, but at a considerable distance from the lake Titicaca.' Now, all one can remark upon the different statements of the Doctor and Captain Blanckley is simply this, that the 'elevated tract of land' of the latter is not included geographically in 'the high plains' of the former;

and as Captain Blanckley has added, that the spot where he exhumed them is at a considerable distance from the lake Titicaca, it is fair to presume that his discovery refers to some locality nearer the sea; an opinion which I should consider to be correct, as he was only a casual explorer, not able to venture far from the ship of which he had the command.

" Dr de Tschudi considers that I have been 'too hasty in determining the race of the nation to which the skull belonged.' All I have said upon that question is as follows:—'This peculiar race were in all probability the aborigines of the country, and it is probable that these mummies may be the relics of some of the last of the Titicacans;' so that it must be observed that I have not determined—I have but suggested, and the question is left entirely open for the more competent to argue.

" In the last place, Dr de Tschudi alludes to the mixed race, recently from the intermixture of the aborigines with the followers of Manco Capac, as if I had referred the mummies to them or their descendants. In this he has completely misunderstood me, as will be apparent from what I have just stated, and from this which I now quote from my original papers:—'I would suggest that the adult skulls of Titicacans, in the Museum of the Royal College of Surgeons, are of this kind, the one possessing all the peculiarities of the race in its unalloyed form—the true Titicacan; and the other being of a spurious character, resulting from the union of the indigeæ with the settlers of Asiatic origin, the companions of Manco Capac of traditionary fame.' "

The Mongols. By Bayle St John, Esq.*

(Communicated by the Ethnological Society.)

The Mongols belong to that vast family of nations which inhabits the eastern, central, and perhaps northern, divisions of Asia. But they are most intimately connected with the Tatars—so intimately, indeed, that it would often be difficult to distinguish the descriptions given by travellers of the two people, were it not for certain characteristics which the subject state of the one, and the independent condition of the other, have impressed upon them. Not many centuries ago, there appears to have existed so little difference between the Tatars and the Mongols, that their names became convertible terms. Carpin furnishes more than one example of this; and he expressly asserts that the Yeka, or Great Mongols—the Su-Mongols, commonly called Tatars—the Merkats, and the Metrits—resembled each other so much in form and language, that the only division he could perceive was into countries and provinces. Perhaps we ought to consider the word Tatar as a generic term, and apply it indifferently to all the inhabitants of Central Asia, including the Independent Tatars and the Mongols as the principal sub-divisions. The traditions of these people represent them as descendants of two brothers—according to some old travellers, Gog and Magog; and, indeed, if we base our views on the opinions of the tribes of Central Asia themselves, we must recognise them every one as closely related. Isbrants Ides informs us, that all with whom he had come in contact seemed anxious to assert their community of origin. It is well known, moreover, that the Turks are a branch from the same stock.

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* Read before the Ethnological Society, 24th January 1844.

In the present paper, however, I intend to confine myself to the Mongols properly so called—that is to say, the descendants of the race which, under the banner of Genghis Khân and his immediate successors, overran and subdued the greater part of Asia, and the north-east of Europe. According to Rashíd-eddín, the name (which, used as an adjective, signifies "valorous," "courageous") was first bestowed on the numerous progeny of Alung-goa, mother of Budantzar, tenth ancestor of Genghis Khân, about the year 1000. It must have been afterwards applied by extension to the subjects of Budantzar; for at the birth of his illustrious descendant, the Mongols were already a powerful people. Subsequently, many tribes of kindred origin assumed the name, in order to claim relationship with the conquerors of the thirteenth century.

The *Ghers*, or felt-tents, of this pastoral people were originally pitched amidst the mountains and forests on the south-eastern banks of Lake Baikal, round the mouth of the Selinga, which, flowing from the very heart of Mongolia, seemed to tempt them upwards to the land which they afterwards occupied. They settled also in the islands of the lake; and Olkhon is still inhabited by their descendants (the Buriats), who possess fine herds of cattle, cultivate the ground, which they carefully irrigate by little runnels derived from their rare springs, hunt wolves, bears, and squirrels, and cross over to the southern shores of the lake to capture the seal. Previous to the promulgation of the Lamaic religion among the Mongols, the waters of Baikal, and the mountainous island I have mentioned, seem to have monopolised a considerable portion of the veneration of the people of this part of Central Asia. Olkhon was, and is indeed still by many, believed to be the habitation of a god invested with certain ill-defined attributes of terror; and the lake itself has been endowed with consciousness and a due sense of its own importance. It will not, it is said, submit to receive the contemptuous epithet of *Osera*, "sleeping or stagnant water," and stickles for the appellation of *Dalaï*, or "sea." By its very nature, however, it is precluded from avenging its dignity on those who insult it from the land; but woe to him who ventures to treat it ignominiously, whilst sailing or sliding over its surface! Tempests blow, waves rise, the ice cracks, and the ingratitude of the traveller is often punished with death! An adventurous Russian resolved once to try the temper of the liquid divinity, and, when he had reached the centre, poured out a glass of brandy, in which he drank the health of the Christians of Europe, calling upon the lake, by the opprobrious epithet of *Osera*, to be his witness. The terrified natives every instant expected to hear the first howl of the hurricane, but the weather was more than ordinarily serene, and they urged their sledges hurriedly towards *terra firma*, wondering at the unusual forbearance of the insulted lake!

It was in such a situation, and in the midst of such superstitions, that the tribe of Mongols grew up, scarcely keeping pace with its neighbours in knowledge and civilisation, until the birth of the Great Temugin—by some, derived from a smith—by others, from an ancient family who introduced the use of forges into the country—by the Chinese, from the blue wolves and white goats, which they assert to be the ancestors of all the Mongols; but, as I have already observed, by Rashíd-eddín and other credible authorities, from Budantzar, son of Alung-goa. This is not the place to relate the exploits or estimate the character of that celebrated conqueror. I shall merely observe, that after spreading on every side with astonishing rapidity, massacring or enslaving surrounding nations, the Mongols beheld their brilliant but brief period of conquest fade away, and were once more confined to their steppes and plateaus, and reduced to live on their herds and a scanty agriculture. The establishments they made in foreign countries, if we except China and Hindústân, had none of the elements of duration. They could storm and sack fortified places, win pitched battles, build cities in the midst of wildernesses, but they could not, at least in most instances, conceive and execute any plan for keeping the fertile districts they overran in anything like lasting subjection. It remained for their brethren the Turks to perfect a system by which a barbarous tribe, such as they were, could establish a permanent sway over a civilised though effeminate empire.

The Mongols, however, were soon driven back from their splendid acquisitions; or, rather, as soon as fresh accessions to their forces ceased to flow from their original seat, they melted into the populations they had conquered, without influencing in any perceptible degree their form of government, their manners, or their religion. This last, indeed, the Mongols in most cases received from the conquered.

There are two periods in the history of Mongolia since the days of Genghis Khân: the first extends through the thirteenth, fourteenth, fifteenth, and sixteenth centuries; the seventeenth was an age of transition; the second period continues to our own day.

During all this time there may be observed a gradual revolution in the manners and character of the Mongols, amply accounted for by the changes in their political condition and religious ideas. In the first place, we behold the imperfect civilisation they had attained to under Genghis Khân, rapidly giving way before the influences of their climate and the configuration of their soil. It was not of native growth, and never took firm root among them. They soon relapsed into their original barbarism, and split into tribes, the number of which constantly increased, whilst each claimed to be governed by a descendant of the Khân Temugin. Meanwhile, however, the Kootooktoo, the great Pontiff of Mongolia, gradually extended his influence with the increase of the Lamaic religion; so that, at the period of his voluntary submission to China, he was enabled to carry with him a great part of the whole population. At this very period it was calculated, by shrewd observers, that, had the Mongols known their own strength, they could once more have conquered, not only China, but the Manchûs themselves, with the greatest facility. Instead, however, of refusing to submit to the yoke, the greater number—I except, of course, the Sungarians, who made a bloody resistance—yielded without a murmur; whilst those who still asserted their independence, contented themselves with continuing their predatory incursions, both on the Siberians and Chinese, and assaulting the caravans that passed to and fro. Their attacks were conducted in a very peculiar manner. It was their custom to set fire to the grass round the camps, and endeavour to burn out the travellers. They were often, however, too timid or too weak to follow up their attempts, and their intended victims escaped with the loss of a tent or so, or perhaps of a camel or a horse; but tracts burnt up for the space of two days' journey frequently exhibited the mischievous consequences of their proceeding.

Since this time China has gradually consolidated her power; and her manners, considerably modified it is true, have been adopted by the Mongols, who are now distinguished by gentleness and docility; whereas formerly they were ferocious, intractable, cruel, and insolent. Martini remarks that they are still subject to sudden outbursts of anger, in which case neither their father nor their mother is safe from their wrath; but in general it is acknowledged that their character is good. It is difficult to say, whether the beneficent precepts of the Lamaic religion, or the influence and laws of China, have had most share in the production of this marked change. At any rate, certain it is that all travellers unite in asserting the superiority of the character of the Mongols to that of their fellow-subjects within the wall, who are equally submissive, but far less kind and hospitable to strangers. This superiority is strikingly evinced by the gratitude which the pastoral people feel and express for the smallest present, whilst the rapacity of the Chinese is never satisfied, and is so intense that thankfulness for past favours is almost entirely swallowed up by cravings after new.

At the same time it must be remembered, that, in industry, the Mongols are extremely deficient, whilst in this the Chinese excel. The latter are averse to leaving a single foot of land uncultivated; whereas the former can scarcely prevail on themselves to sow a little millet, barley, and wheat. This has been accounted for by Timkowski in the following manner:—"The sterility of the steppes obliges the Mongols often to change their habitations. Always on the look out for pasture, they are frequently obliged to pass the summer in places very distant from their winter and spring encampments, and consequently to leave their cultivated fields for a long time." But natural idleness has much to do with their agricultural slovenliness. Even in those quarters, between Kiakhta and Urga, for example, where wood and pasturage abound, they neglect to prepare dwellings, or lay up provisions for the winter, contenting themselves with carelessly heaping up a few stacks of hay. Accordingly, when the snow falls and the cold strengthens, their cattle are attacked by disease, and perish in incredible numbers. The Lamas, on the other hand, are active cultivators; and the church lands of Mongolia, instead of being, as in some of our colonies, impediments to civilisation, might, if the people possessed any of the necessary qualities, form nuclei for the successful exertion of agricultural industry.

The portrait of the Mongols has been painted with various colours. When they were objects of dread to the nations of the earth, words could scarcely be found to describe their hideousness; and the pictures left of them are rather those of devils than men. This prejudice has been imbibed by Bory de St Vincent, who says they are the most hideous of the human race, though he is of opinion that one of their branches, the Turks, became the most beautiful, by migrating into the balmy Ionia, Macedonia, and Greece, and by mixing with the Circassians and the descendants of the ancient Hellenes. It appears, however, that the reports of the ugliness of the Mongols have been greatly exaggerated. Timkowski observes, that many of the women, with their clear complexions, cheerful countenances, and lively, animated eyes, would be esteemed handsome even in Europe; and the Baron de Bode assures me that he has seen Tatars who possessed great personal beauty.

From this, however, it must not be understood that I intend to break a lance in favour of the peerless charms of the Mongols, male or even female. What I mean is, that they are very far from possessing the diabolical assortment of features which has been attributed to them; and that their countenances do at least exhibit a capacity for beauty. Among the principal characteristic features are a slightly pointed head and chin, and high or rather wide cheek-bones. The bare knowledge of these facts, unaccompanied by personal experience, has induced some naturalists to compare the face of the Mongols to a lozenge; but that this resemblance is arbitrarily traced, will appear from the fact that Timkowski, who had seen thousands of specimens, expressly says that their face is round. Their temples are slightly hollow, and the upper maxillar is square, whilst the lower, on the contrary, is somewhat pointed. Like the Chinese, too, the upper teeth of the Mongols project, so as to rest sometimes upon the lower lip, whilst the other range inclines rather inwards. This peculiarity of construction influences greatly the pronunciation of their language. But the most remarkable traits in the physiognomy of the Mongol, are the oblique position of the eyes, and the distance between them, by some exaggerated to more than the breadth of a man's hand. The former characteristic is common to the Chinese, whom I believe to be the first Tatars who came down from their plateaus to settle on the plain, being tempted by the fertility of the banks of the Hoang-ho. In later times the same impulse led to the frequent conquest of the country, and the transformation of successive tides of invaders into peaceful, and at length effeminate citizens. The Malays, also, have the inner corner of their eyes depressed, and the outer raised towards their temples; and Lesson observed the same peculiarity in some of the islanders of the Indian Archipelago.

Whilst on the subject of the eyes of the Mongols, I may observe that they are deep-set and lively,—"inconstant" is the expression of an old writer,—and that their iris is almost always black, though said to be blue by Bory de St Vincent. This incorrect writer asserts, moreover, that these people are furnished with an ample growth of beard, especially on the upper lip; whereas all travellers who have visited Mongolia concur in representing their faces as covered with a very tardy and scanty crop of hair. They admire, however, and envy this element of manly beauty; and, when chance bestows it upon any of their countrymen, look upon him with extreme veneration. A stranger, too, may be sure of respect in exact proportion to the length of his beard. Whiskers, which are more common, are less prized; whilst the hair over the forehead and temples, in obedience to the caprices of fashion, is shaved, the rest being braided into a tail which hangs down the back. Even the varieties of the toilette form curious subjects of study for the ethnologist. This simple method of disfiguring the countenance has succeeded another far more complicated but no less effectual, which has been described with greater minuteness than perspicuity by the old travellers. We may gather, however, from their accounts, that the period of the greatest political splendour of the Mongols was coincident with their greatest elaborateness of dress; and that, like individuals, they have become more careless in proportion as they have sunk in the scale of fortune. It is well known, that after the task of conquering China had been accomplished by the Manchûs, they nearly forfeited their new acquisition, by imposing their head-dress as well as their laws upon the vanquished. They insisted on the adoption of the fashion I have described. The empire was convulsed from one end to the other. A general insurrection was for a while expected, but the conquerors were firm; and the Chinese furnished the strongest possible testimony of their humiliation, by consenting to change their customs as well as their masters. It may be that it was the desire of the Manchús to prevent the repetition of the notorious influence exercised by the Chinese on the former Tatar invaders. A second attempt of a similar tendency was made in later times by the Emperor Kien Long, who caused 5000 Manchú words to be substituted for as many Chinese ones, forbidding the use of the latter under pain of corporal punishment!

The hair of the Mongols is black, and naturally by no means scanty or short. Among the neighbouring Tunguses instances have been met with of hair of extraordinary length. A Russian ambassador mentions a man whose locks measured four yards, and whose son promised in this respect to emulate his sire.

The complexion of the Mongols is sometimes described as dark-yellow, sometimes as deep olive. The truth seems to be, that it is rather sallow and tanned by the sun. The children are frequently mentioned as having ruddy cheeks: and the rosy countenances of the women are also dwelt upon.

The stature of the Tatars generally is moderate. Their legs are remarkable for their shortness; their feet also are small; and their knees are slightly bent out. Their thighs are thick, their shoulders broad, their waists small, their arms long and vigorous. The peculiarities of their lower limbs may result from their equestrian habits; the strength of their arms is very possibly derived from the constant use of the bow.

It is natural that a slight notice of the country inhabited by the Mongols should succeed the description of their physical organisation. Without believing in the theory of autochthoneity, I consider man to be in some measure the creature of the hills, valleys, lakes, rivers, winds, storms, and sunshine of his native land. All these participate in the formation of his character. It is in this sense alone that I understand that the Tatar race traces its origin to the Altai chain of mountains. There was the cradle of its future individuality. In the regions to which its various subdivisions migrated, new elements were added by degrees. Not the least remarkable instance is that of the Mongols.

Their present country occupies the sides and summit of a vast swell in the surface of Central Asia, broken up into hills and valleys, and intersected by a few large rivers and numerous small streams. It is crowned by the great desert of Kobi, or Shamo, as the Chinese call it, one of the wildest and bleakest regions of the globe, of still unknown extent and undefined limits, though parts have been more than once explored and described. In some places its surface is undulating, like that of the rolling prairies of America; in others it is rough, broken up by ravines, and gullies, whilst frequent plains are met with, covered with pasture. The hills are generally clothed in a mantle of dark *búdurgúna*, which resembles young oak-shoots, and are often inhabited by such prodigious numbers of mice, that the horses' feet sunk at every step into their burrows.

Among the ever-recurring features of a Mongolian landscape, are the salt-lakes, with their white incrustation, and elegant fringe of slender reeds. Many of these are met with in the vast sea of sand and flints which stretches north of the Tsakhars.

But we must not consider Mongolia under the most unfavourable aspect only. In many quarters it is highly fertile, especially near the Great Wall, where the climate has been compared to that of Germany. The banks of the Boro, the Shara, the Iro, and other large rivers in the northern section of the country, abound in pasture, and there occurs here and there land admirably adapted for tillage.

In one part of the desert of Kobi, there is an eminence, which, seen from a distance, appears like a forest. As you approach, however, an extraordinary *lusus naturæ* is observed. Here is beheld an immense altar; there a sarcophagus. Now is seen a lofty tower; then the ruins of a house with a stone-floor. The rock, a decomposed granite, lies in large masses, from three to nine inches thick; in some parts the *Robinia pygmæa* grows thick on the surface; no other plants are seen, and the soil around is sandy. The Mongols declare that much loadstone is found in this place; and if any one approaches with a gun, it is strongly attracted. In Mount Darkan is said to be preserved the anvil of Genghis Khân, composed of the peculiar metal called *buryn*, possessing the properties of iron and copper, being at once hard and flexible.

One of the peculiarities of a Mongolian landscape is, that almost every considerable eminence is surmounted by an obo or altar, consisting either of a heap of stones, a mound of earth or sand, or a construction of wood, generally of colossal dimensions. These altars are raised under the direction of a Lama, with many solemn ceremonies, and are constantly visited for the purpose of prayer, or the presentation of offering. Every passer by alights from his horse, places himself south of the obo, with his face to the north, makes several prostrations; and, having breathed his humble supplication, and deposited his gift, rides away, satisfied with the performance of his duty. Tufts of horse-hair are the most frequent offerings, the object of which is generally the preservation of the pastoral riches of the Nomades. Similar ceremonies, with a similar object, are performed by the Yakoutes, in the worship of the Spirit of the Woods.

The climate of Mongolia is generally cold, but in some places, and at certain times, the heat is excessive. Kiakhta itself is 2400 feet above the level of the sea, consequently, higher than all the towns of the Hartz and Swiss Alps; and there is a continual rise from this place to Urga.

It is well known, that Mongolia is politically divided into several principalities, each recognizing the sovereignty of the emperor of China. This is not the place to enter into any detail on the arrangements by which government is carried on. I can only say, that they ensure the complete subjection of the Mongols; and that even the Chinese themselves now feel that their Great Wall is superfluous. Previous to the annexation of Mongolia, this stupendous fortification seemed always in a state of siege, so numerous were the soldiers that passed to and fro along it. It now winds its deserted line along the valleys, up the sides and over the crests of the mountains, like a railway started without sufficient capital to keep it open.

The Lamaic religion is one of the chief instruments for keeping the population in order. Its own natural influence is to render the people who profess it mild and gentle; but its ministers are, besides, under the complete control of the celestial emperor, who even directs the inspiration of the Kootooktoo, or Pope of Mongolia.

There is one point in the ancient civilisation of Mongolia, which may be worth noticing. Europe, towards the close of the middle ages, was filled with reports of vast cities in this part of the world, among the principal of which was Karakorum. But modern geographers deny that these cities had any real existence, at least, with the circumstances of grandeur which have been attributed to them. Malte Brun observes, that no ruins remain to attest the former splendour of Karakorum; and that "the Mongols have never been sufficiently numerous, or sufficiently rich, to build cities worthy of the name." But even in the desolate steppes of Kobi there occurs the fragments of former architectural magnificence; in one place they encumber the slope of a mountain for the space of two wersts. They are all of stone; the remains of temples, altars, and other buildings of colossal dimensions, present themselves on every side, covered with grass and moss; in some cases the foundations only are of granite, whilst the superstructure is brick. Clay, mixed with gravel, was used as mortar; the clay has now disappeared, and the gravel alone remains. Some of the buildings are round, and adorned with cornices; in the temples are empty vaulted niches, broken bits of a green stone strew the courts, and troughs of the same material also occur.

For a space of four wersts beyond the cluster I have described, similar remains are visible, though more thinly scattered; and tombs, towers, and deserted walls appear on every side. There cannot be a doubt that on this spot a vast population once swarmed; for in all probability the most important structures have alone survived, those of a humbler character having been constructed of a more perishable material. "These ruins," says Timkowski, "formerly inhabited by a descendant of Genghis Khân, now serve as a retreat for the flocks; the Mongols seldom visit the monuments of their former splendour and independence."

I can hardly understand how, after this, M. Bory de St Vincent could have asserted of the race, in which he includes the Mongols, that they have never attempted to build cities, "Nulle part ils n'ont bati des villes."

But I have not as yet alluded to all the authentic accounts of ruined cities in Mongolia. The Russian ambassador Isbrants Ides described no less than three in the seventeenth century, full of fragments of statues of kings sitting cross-legged (perhaps Budhist idols), and surrounded with an earthen rampart. These, it may be said, were not cities in our sense of the word. They were rather nuclei for population, consisting chiefly of public buildings; but I question whether the wooden habitations with which they were surrounded, were not at least as durable as the brick houses of London at the present moment; and whether any other traces will remain of this great metropolis three or four hundred years after its total desertion, than its churches, prisons, parliament-houses, and other public edifices.

However this may be, certain it is, that the Mongols have generally manifested a peculiar predilection for temporary habitations, tempted thereto by the nature of their steppes, and the occupations to which they are compelled to addict themselves, as well as by their own inclinations fostered by their mode of life. The skeleton of their tents is generally made of osier, the cross-pieces being tied together with small thongs. The rafters of the roof are long poles, which meet at the top, leaving a small opening for the smoke. The covering of this frame-work consists in summer of one, in winter of three, layers of felt, manufactured of wool and horse-hair, procured by cutting off the manes of the foals in their first year, and that of some of the horses every spring.

The real Mongol name of a tent is *gher*, though travellers generally use the Siberian terms *kibitka* and *yourt.* On entering the low and narrow door, which is always turned towards the south, you observe on the right hand, near the entrance, the place reserved for the women. Aged persons have carpets of felt, with patterns worked in them, to sit on. The rich import these luxuries from Persia or Turkestân. Opposite the entrance is a small table supporting copper idols and various utensils for the offerings. On the right hand of this stands a wooden bedstead covered with felt; to the left are trunks, boxes, &c., for clothes. All the Mongols sit cross-legged on the ground, so that chairs and couches are dispensed with. Their dwellings are mostly very small, though those of the rich are comparatively spacious; and in some instances several tents are joined together, so as to resemble the various apartments of one house. These *ghers*, as they themselves confess, are often inadequate to protect them from the cold, so that the little children are sometimes completely wrapped in furs and skins.

The dress of the Mongols generally is in summer a long robe made of nankeen (like their shirts and other under garments), or coloured silk and satin, generally dark blue. Their cloth cloaks are usually black or red, with yellow button-holes. A leathern girdle, fastened with silver or copper buckles, serves to hold a knife, flint, and steel. Their silk caps are round, and trimmed with black plush; three long red ribbons hang down behind as ornaments, and produce a very beautiful effect, as they wave and flutter in the wind. Their thick-soled boots are made of leather. In winter they are protected from the inclemencies of the season by long pelisses of sheep skin, and caps trimmed with the same material, or the fur of sables, foxes, or marmots.

The women dress in many respects like their husbands. The old travellers assert that they could see no difference. But at present, if there be not much distinction in form, the female costume is remarkable for its superior richness. The robes of the wealthy are often of the most beautiful blue satin, their caps of sable, their silken zones interwoven with silver, and studded with large carnelians. Even the saddles of their horses are covered with these precious stones. They divide their hair into two tresses, which fall on the breast, and are adorned at the extremities with small pieces of silver, coral, pearls, and precious stones of different colours. Coral is much prized in Mongolia, and is very dear.

The Mongol bridles, saddle, and harness, are often ornamented with copper, rarely with silver. Bows and arrows, with a short sword, are the favourite arms of the country, as they have always been among pastoral nations. We may suppose that the custom which prevailed anciently in China, of hanging up a bow and arrow before the door of a house at the birth of a son, was a remnant of the nomadic habits of the people. Muskets and rifles are only used by hunters, who obtain their powder, shot, and balls, from China.

Milk forms the staple article of food in Mongolia, being used as a beverage in its original state, and afterwards eaten when transformed to butter and cheese. This light food may account for the activity, as well as the lack of muscular vigour of the people. A Cossack is more than a match for a Mongol; but the latter, even when arrived at the age of sixty, will ride, it is asserted, two hundred wersts in a day without being fatigued. In summer they drink a kind of brandy, which is extracted from milk. I may here remark, by the way, that smoking is extremely common. Meat is rarely eaten; and then mutton is preferred. No game is touched, except on pressing occasions, but the wild goat and the wild boar. Fish are protected by superstition. In extreme cases they will eat the flesh of camels, horses, and even of animals that have died of disease; in which, I suppose, they would be imitated by every European under similar circumstances, though our fastidiousness might perhaps lead us so form a different opinion of what constituted urgency. Water is rarely tasted, brick tea being the favourite drink. This, indeed, is almost invariably the contents of the cast-iron kettle which swings over the fire of dried dung; and any traveller who passes by, provided he be furnished with his own wooden cup, sometimes lined with silver, may enter and quench his thirst. This beverage, called *satouran*, is generally rendered palateable with milk, butter, and salt. A little flour fried in oil is sometimes added. What is usually denominated brick-tea consists of the dry, dirty, and damaged leaves and stalks of tea thrown aside in the Chinese manufactories, pressed in moulds, and dried in ovens. The Chinese will never drink it themselves. But the Mongols, the Buriats, the Kalmucks, and the Siberians, use it to excess. The latter, indeed, are said to weaken their constitutions by this means.

The small, fat, buffaloes of Mongolia are generally black, and their tufted hair gives them an extraordinary appearance. The sheep, which furnish abundance of milk, and whose excellent meat is spoken of by Martini with the relish of a connoisseur, are white, with long black ears and very large tails, like those mentioned by Herodotus and Ælian. They belong to the second class enumerated by these writers, and are not those which, from the length of their tails, required a little carriage to prevent them from dragging on the earth,—the peculiarity consisting rather in extreme breadth. The Mongolian horses are small, but vigorous and spirited. Their head is remarkably short: their hoof narrow.

Were any accident to deprive this people of either of the three species of animals I have described, a great revolution would necessarily be effected in their mode of life, and considerable influence exerted on their habits and physical organisation. The gradual destruction of the rein-deer in Siberia, within these last two or three centuries, has brought many changes into the manners of that country, besides introducing the use of dogs; but the loss of the buffaloes, the sheep, or the horses, would be far more influential on the fortunes of the Mongols. That the contingency which I have supposed is by no means an improbable one, is shewn by the parallel case of the rein-deer in the country immediately to the north; and about twenty-five years ago, the whole steppe of Kobi was visited by such a mortality among the domestic animals, that some proprietors of five hundred horses had not above twenty left, and others who possessed two hundred, had saved only four. It seems, certainly, at first sight, by no means likely that the breed of horses should be destroyed in Mongolia. Still, admitting even the possibility of such an occurrence, we are at liberty to speculate on its consequences.

In Siberia, it has been observed that those tribes which have lost their rein-deer have sensibly deteriorated, and afford a striking contrast, by their humility and weakness of character, to the martial disposition and proud bearing of the more fortunate people. I have no doubt that the Yakoutes, before they were reduced by Russia, and had begun to employ dogs instead of rein-deer, offered far more points of resemblance with the Tchuktchis than at present. A similar result would perhaps arrive, were any portion of the Mongol race deprived of its horses, its buffaloes, or its sheep. But, in addition, some of the most striking of their physical characteristics might become gradually obliterated.

To convince ourselves of this, we have but to reflect on the extent of the influence exerted by their peculiar mode of life on the Mongols, and on the determining causes of this mode of life. In the first place, their nomadic habits, and all the modifications of their character and structure resulting therefrom, are attributable to the necessity they are under of seeking support for their herds and their flocks. Their wandering life, to which Lucian compares that of a gourmand continually passing from one part of a table to another in search of a variety of good things, is especially inimical to steady industry, and must induce a certain tendency to vacillation and inconstancy, combined with general indolence and momentary displays of energy. One of the wisest of ancient writers asserts this character to be distinctive of a nomadic people. Should the Mongols ever be induced, by the accident I have supposed, or any other reason, to settle in their fertile valleys and plains, the natural result would be, the disappearance of this quality—this restlessness, I mean, and love of change, and unsteadiness, and proneness to indulge in speculative migrations, as well as aptitude to grow disgusted with late acquisitions,—from which most of the splendid achievements, and most of the misfortunes, of the race have proceeded. That there is arable land in Mongolia sufficient to support an agricultural population of two millions (the estimated number of the present inhabitants), I have no doubt.

I have already made some observations on the milk-diet of the Mongols; but there are a few facts which I have purposely withheld for this place. Even so far back as the time of Homer, the habits of the Scythians or Tatars were so well known, that they won for them the appellation of Milk-Drinkers; and all nomadic nations have exhibited the same propensity. It is curious to remark, that Coxe, in describing the wandering shepherds of the Alps, asserts that they live on cheese, curds, and whey. The Mongols, as we have seen, like the ancient Æthiopians, indulge occasionally in meat; but milk, and the substances extracted from it, still form their staple articles of food. Mares' milk is generally preferred,—not, as was believed in the last century, because the cows will not suffer themselves to be milked, but because, on turning sour, it acquires a slightly inebriating quality. When in this state, Pallas informs us, it is called *koumiss*—the *kosmos* of Rubruquis, the *kemuls* of Marco Polo, and suggests Coray, the *oxygala* of Strabo. It is from this *koumiss* that the brandy I have already mentioned is manufactured. In winter, says Witzen, when the mares are less lactiferous, a beverage composed of snow water, honey, and millet, is substituted. It is obvious that the constant use of food so peculiar, for a long succession of ages, must have strikingly influenced the physical character of the Mongols; and that the substitution of a vegetable diet, which would be consequent on an alteration in their mode of life, would work considerable changes in them.

But on the nomadic mode of life depends, also, the constant use of horse-exercise, which I conceive to be one of the principal causes of some of the characteristics of the Mongols. Coray, in his learned notes on Hippocrates, enlarges on the diseases to which equestrian nations are peculiarly liable. On this theme I am not competent to enter; but it is easy to understand how, in this way, their moral character may be affected. Not, however, to lengthen out this speculation, some of the distinguishing characteristics of the Mongols,—I mean the shortness and outward curvature of their legs, and the smallness of their feet,—would, I think, entirely disappear as soon as their present mode of life should be changed.

Description of a Totally Reflecting Prism, employed for illuminating the open cavities of the Body; with a view to facilitate the examination of Disease, and the application of remedial means in such situations; illustrated with an Ear "Speculum" or Prismatic Auriscope, adapted to this method of observation. By Adam Warden, M.D., F.R.C.S.E. (With a Plate and Woodcut.) Communicated by the Royal Scottish Society of Arts.*

Having been present on repeated occasions at meetings of the Society of Arts, and appreciating highly their efforts to give an impulse to useful observation, and to its practical application to general purposes, I beg leave to present to their notice a method of illuminating the open cavities of the body, whereby the examination of disease and the application of remedial means in such situations may be facilitated.

As I had heard Monsieur Charles Dupin express himself to the Society of Arts of Paris, "Here, in the doctrine of parallel lines, the weaver and the carpenter are to see the secrets of their own art, and in the various expositions every artisan is to catch the application of the doctrine of his trade;" so, in listening, as a casual auditor at a late meeting of this society, to some notices of the useful application of prismatic reflection, an adaptation of a prism to the apparatus of surgery suggested itself to me, and is now submitted to inspection.

A short time before the meeting of this society above referred to, my attention was especially awakened to the difficulty attending the management of diseases in the open cavities of the body, by the experienced imperfections of the existing apparatus of aural surgery. The object of my present communication is to explain my attempt to improve that apparatus, by a new method of throwing light upon the parts to be examined, and it will readily appear that the arrangement proposed is equally applicable to any of the other open cavities of the body. The manifold importance of the diseases of the ear, as affecting the valuable sense of hearing, and in their more serious forms even endangering life itself, will be conceived, when it is mentioned that Valsalva, one of the most eminent anatomists of the last century, devoted sixteen years of a laborious life to their investigation, and to the composition of his treatise on the subject. His biographer, Morgagni, mentions that he performed an incredible number of dissections in this research. The great proportion of the diseases of this organ, and those of the most remediable kind, have their origin in the external auditory canal, and the tympanum at its termination—the vascular texture and the situation of those parts exposing them to the first attacks of disease, and, whence, in its unchecked progress, it is propagated to the complicated interior structures, to the bones and the brain.

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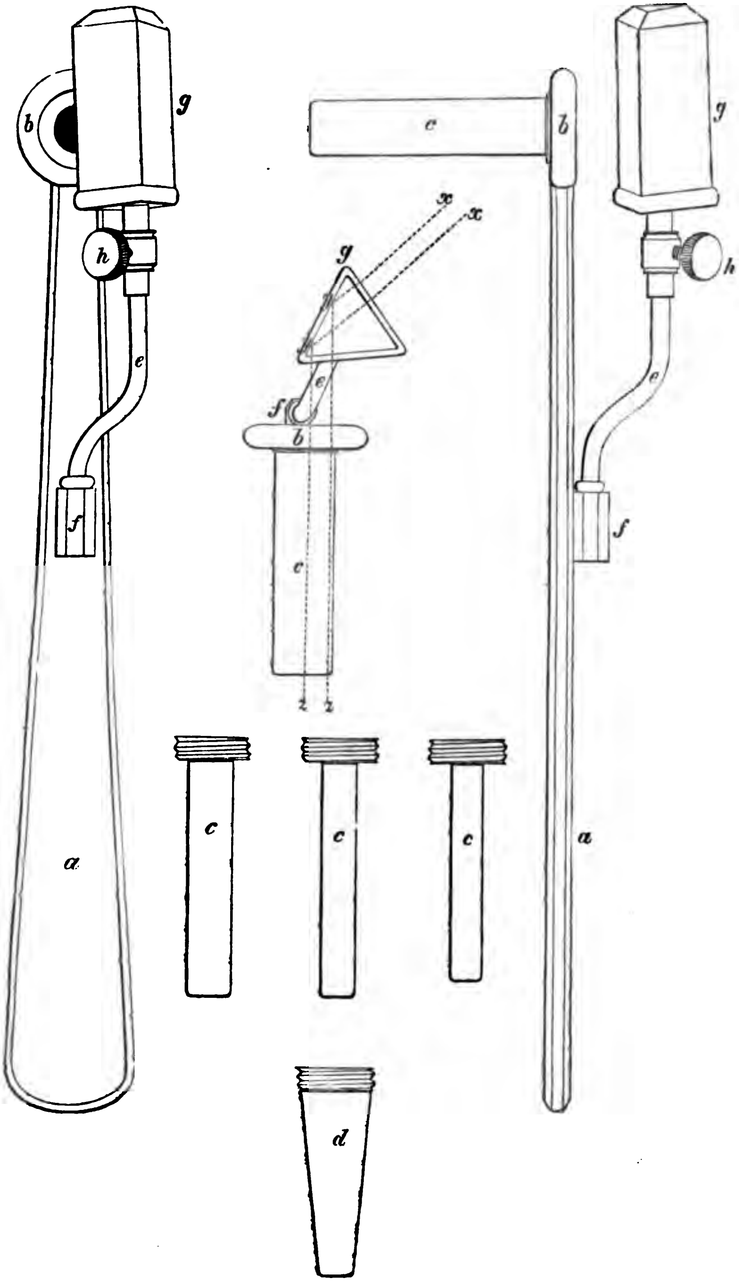
* Read and exhibited to the Society on 22d April 1844.

The external auditory passage exceeds an inch in length, is curved in its course, and is commonly so beset with hairs as to prevent the view of its inward track and termination; but by the aid of a straight canula, or the common *speculum*, these obstacles to observation are readily obviated; sufficient light, however, to illuminate the passage, still remains the one desideratum, without which, to exhibit the actual condition of the membrane of the tympanum, the surgeon cannot pronounce as to the importance or curability of disease, nor resort, with confidence of its safety from danger, even to the popular remedy of the syringe. The instrument submitted to the Society, and represented in the Woodcut, is constructed upon the principle of illumination derived from prismatic reflection. It consists of separate portions: a straight handle, *a a*, five inches in length, terminating in a ring, *b*, of half an inch internal diameter, the ring grooved in its interior as a screw. To this screw are adapted four canulæ or straight tubes, *c*, of one, two, three, and four lines calibre, and another, *d*, of a funnel or tapering shape, applicable to the dimensions of the auditory canal at different ages and under different degrees of constriction resulting from disease; the wider mouthed canula is intended to be employed for preliminary exploration and removal of any accumulated cerumen obstructing the passage of the light, also for affording a proper field for the passage of instruments and other topical manipulation. The canulæ are an inch and a quarter in length, and terminate in blunted edges, to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prevent injury in their passage into the ear. From the middle of the straight handle, and at an angle with it of about 20°, arises a curved branch, *e*, moveable in a pivot joint at *f*, toward either side of the handle. This branch forms a stalk, on which a prism of flint glass, *g*, is perched erect, to the level of the opening of the affixed canula. The prism rests in a metallic socket, and is made to revolve on its own axis at the touch of the finger, or to remain fixed in any desired position by the aid of a small clamping screw, *h*. The instrument is thus complete for use. The canula is to be introduced into the ear to be examined, the patient being seated by a table, having a good light of a gas jet or argand burner at a convenient distance to one side. The surgeon being placed opposite to the ear to be inspected, a face of the prism is turned towards the light, and it is made to revolve until the luminous spectrum is conveyed to the bottom of the canula, and to the surface sought to be observed. There is no difficulty in the adjustment of position, when the relations of the light and the object are ascertained by a little experience; and when this adjustment is made, the full and clear illumination of the object is at once obtained, and with a degree of brilliancy proportioned to the quantity of light employed in the particular observation. Where different circumstances require it, the intensity of the light may be artificially increased to any desired extent.

The principle or theory, as already mentioned, consists in total reflection. The light is received by one side of the prism, is reflected from the second side, and emerges by the third side to the object illuminated, as represented by the dotted lines *x y z*, and thus its view is revealed to the eye. The light afforded is nearly equivalent to the same candle or gas-light applied to the page of a book, or other familiar uses, so little of it is lost in its passage through the prism. The illumination is not preternatural or dazzling, such as would alter the real features of disease, but natural, and such as the eye is familiar with. The advantage of this unconcentrated natural light can only be fully appreciated by professional eyes; and I am persuaded that any method of concentrating light by lenses or converging mirrors, substituted for the prism, would not increase the serviceableness of the instrument exhibited for medical examinations, but the reverse; and of this I speak from sufficient experiment.

The Society will understand that whilst this instrument is constructed so as to afford the utmost amount of light which the dimensions of the passage of the ear can admit, it is not intended to supersede the use of the speculum which generally bears Dr Kramer's name, the utility of which, in so far as that extends, is established by the concurrent experience of the profession; neither in the other applications of prismatic illumination which present themselves to me, would I be understood to depreciate existing apparatus, when I suggest such modifications as may increase or extend their efficiency. Indeed, it has been my aim, in constructing the other adaptations of the prism, to frame these so as to be a ready appendage of all the different forms of specula in the hands of the profession. The importance attached by professional persons to the existing very imperfect methods of illuminating the ear may be estimated by a reference to Dr Kramer's Treatise on the Ear, translated by Dr Bennet, which describes his ingenious efforts to effect this end in these terms. "In order to obviate the above objections (to all other forms of illumination) as far as possible, I have constructed the following apparatus. The principal part is an argand lamp, with a thick cylindrical wick, the reservoir of oil being placed behind the box next to be described. This box is constructed of tin-plate, the inner surface of which is painted black in order to prevent any reflection of the light. It covers the lamp so as completely to enclose the flame, the lamp-glass passing through an opening in the top. At a convenient distance from the flame, and behind it, against the inside of the back of the box, there is a plated concave mirror. In the anterior face of the box there is inserted a tin tube fourteen inches in length, which is likewise blackened inside, and each extremity is provided with a double convex lens two inches and a half in diameter. The argand lamp throws its powerful mass of light against the concave mirror, whence the rays are reflected through the first convex lens, along the tube and through the second convex lens. The luminous rays are thus collected into an intensely bright focus of the size of a shilling, at a distance from the tube of the apparatus very convenient for the illumination of the auditory passage." Op. cit. p. 92.

This large and incommodious lantern is liable to the general objection applicable to all such means of illuminating the cavity of the ear, that any direct light, even of the sun itself, is unavoidably liable to be intercepted by the head of the observer in so narrow a field; and if a lamp and lens be placed between the eye and the object viewed, not only does the dazzling artificial medium alter the characteristic appearance of disease, but such apparatus, in order to guide any surgical procedure, must be kept strictly in such a position as necessarily to interfere with any convenient manipulation in the removal of foreign bodies from the ear, the puncture of the membrane of the tympanum, the application of caustic, or any other operation. The method of illuminating diseased parts by the prism is not liable to these objections. The cavity of the ear and other deeper seated parts may be inspected with the satisfaction only short of sunlight view, by means of a common gas jet or other light, and with little less facility than the tongue; while by the position of the prism in the apparatus, it presents no obstruction to the procedure of the surgeon,—a circumstance which commends it to the operator in diseases of the rectum and uterus, where the application of ligatures, cauteries, &c., render any increased facilities peculiarly desirable.

To those who are at all acquainted with the progress of the medical art in our own day, it is well known that this has been chiefly owing to the more exact investigation and the more extended knowledge possessed of the alterations which take place in parts affected with disease, and the minute detail of physical appearances which medical authors employ to describe these changes, is well calculated to convey an idea of the great and just importance attached to them. In proportion as we attain to the means of recognizing with precision the actual state of disease in the interior parts of the body, will its successful treatment be insured; and if, by the method here proposed, we can in any case confirm the doubtful testimony of touch, by the farther evidence of sight, which before was wanting, it is obvious that the full force of medical experience may be brought to bear upon such diseased parts with increased confidence and effect. Among the altered conditions produced by disease which come under our observation in the living body, change of colour is at once the most obvious, and in all its diversities the most significant, mark which presents itself. From the first evanescent blush of erysipelas to the inky stains of gangrene, and in the various forms of eruptive disease there is scarcely need of farther intelligence than is gathered by the eye at a glance; and when the softer and more vascular textures within the mouth and the other cavities of the body are the subject of morbid affection, the characteristics derived from the shades of colour, be it of an inflamed, abraded, or ulcerated surface, are still the most distinctive and important to be observed. The tutored sense of touch, so far as that can extend, discerns somewhat, nay much, of the conditions of disease even in the dark cavities of the body; but of colour, it cannot, at the present day, form a better estimate than blind persons are said to have done of scarlet when they likened it to the sound of a trumpet.

The advantage of Prismatic Illumination consists in the opportunity it affords of examining the recesses of the open cavities of the body by light of any desired intensity, and that placed on either side of the observer, so as not to be liable to be intercepted by his shadow, nor to interfere with the freedom of any operative procedure; and by the combination of two prisms, one placed at the external opening of the speculum, the other moveable within it, so as to traverse its extent, disease presenting itself at the opposite extremity of the tube may be fully inspected, while through the transparent sides of a glass tube, or the interrupted continuity of a metallic one, the whole surface of the passage may in succession be surveyed, and remedial appliances conducted to any point affected with disease. Thus the numerous and serious affections of the straight-gut, whose nature is often obscure, and the treatment uncertain and difficult, may derive all the advantages which light and the sense of sight are capable of contributing in other cases. And these advantages are not confined to the very limited extent to which touch can be carried in that particular situation. Those cases of highly-seated stricture which occur at the farther extremity of the straight bowel, and so are removed beyond touch, and all satisfactory management, may, by the method which I have proposed, be brought fully under examination, and have the treatment adapted with the same accuracy as in the more superficial affections.

It is probable that an erroneous idea of the expense of the instruments required for this method of investigation may deter many from making trial of them; but it will serve to remove this mistake if I shall mention that my experiments were mostly made with the materials within reach of all, such as tubes of block-tin, of bone, and of rosewood. At the same time, more expensive materials, as silver gilt, silver plate, or German silver, are the most appropriate, and such as I would recommend. The quality and finish of the glass employed admits of less latitude of choice,—the second or interior prism especially must possess that highly-finished surface which Messrs Adie & Son, opticians, of this city, are so competent to supply.

I shall now, as briefly as possible, explain the parts which compose another of the forms of the Prismatic Speculum, of which a plate is appended (Plate IV.) to this paper. Any difference in the model or size of the instrument will depend upon the specialties of different cases, and the forms of specula which these may demand. Indeed, the same instrument may serve equally for the great majority of cases of disease affecting the rectal and uterine passages. I confine myself to the description of one of these forms of apparatus, that adapted to the examination of the rectum. The first portion, which I have thought it unnecessary to delineate, is a glass cylinder open at both extremities. This is to be employed by the patient as the medium for a jet of water from the syringe in common use, by which thorough ablution and exposure of the diseased surface is to be effected. The second portion of apparatus, represented in two aspects, Figs. 1 and 2, is more complicated, and consists of a metallic speculum (*a*), having a glass tube (*b*) concealed within. This tube is not open at its farther extremity, but is made to terminate in a smooth bulb or obtuse cone, projecting beyond the edge of the metallic tube, as shewn in the plate, so as to facilitate the introduction of the speculum, and defend the surface of the passage from injury. If the disease to be examined is seated in the axis of the instrument, such as stricture, the glass tube is to be withdrawn when it has reached the seat of obstruction, be that at the distance of 5, 7, or more inches, and a beam of light transmitted through the prism (*c*), which is appended to the neck of the instrument, is then to be conducted and made to rest on the part affected, until it be fully and satisfactorily examined, in the same manner as already detailed in describing the auriscope. In passing, it is to be observed, that, while the mechanical method of pressure and dilatation is the most suitable for the treatment of simple stricture, it is obvious that we must forego this plan when the disease is situated beyond cognizance by the finger, the hazard of penetrating the coats of the bowel being a far more likely consequence of the use of instruments in such circumstances than the forcing of the more unyielding parts affected with disease. By the introduction of light, and by obtaining a view of the precise seat and remaining dimensions of the strictured part, the bougie suited to enter this may be selected, and by the first interference relief may be afforded, and progress made in the track of cure.

This much may suffice to exemplify the service afforded by the single prism employed to illuminate a surface at the extremity of a straight tube. For the examination of disease affecting the sides of the cavity, a second prism (*d*) is to be used, mounted on the end of a slender metal rod (*e f*), of a length somewhat greater than the tube, in order that it may terminate externally in a small knob or handle, whereby it may be conveniently moved throughout the extent of the instrument. To obtain the service designed in this arrangement, the speculum inclosing the close coniform glass tube, is to be introduced as before. The metallic part of the instrument represented in the plate consists of two light cylinders (*g* and *h*), one within one another, from each of which a longitudinal section of half an inch, extending nearly the whole length, has been removed. By a semi-revolution of the internal cylinder (*h*), which is effected by a corresponding turning of the projecting ring (*k*), forming the neck of the speculum, the open sections of both tubes are brought into coaptation, as is shewn partially by the dark space in Fig. 4, and thereupon a similar portion of the mucus surface immediately applies itself to the exterior of the glass tube within contained. Illumination being afforded through the exterior prism (*c*), as before explained, the interior one (*d*) in the same manner transmits the light to the surface opposed to its reflecting side, and a picture of this surface, in all the truth of outline and colouring, is, simultaneously, thrown back on the reflecting face of the prism, and so offered to the inspection of the observer. By making the interior prism to course along the open section of the speculum, every portion of the surface exposed may be minutely examined. If the glass tube become dimmed by exhalations or secretions before the survey of a section is completed, it is merely necessary to turn it slightly and to present a clean portion of its surface. Pursuing the same method, neighbouring portions of the lining of the bowel are to be included in the open aspect of the instrument, and examined in the same manner until the survey of the whole cavity is made. Let it be supposed that the open mouth of a bleeding vessel is the subject of search; the longitudinal aperture of the speculum, in that case, may be contracted to a chink, and this may be made to traverse the circle of the passage until the direct issue of the blood obviously corresponds with the opening. If the case be not urgent, after the removal of the glass tube, styptics or a caustic pencil may be applied to the spot, as in common cases; and if the hæmorrhage is profuse, deluging the tube and the prism, we may probably be left in doubt as to the situation of the bleeding vessel to the extent of a quarter of an inch. But it is surely calculated to increase the chance of safety to the patient, to be enabled to conclude, that in one certain small arc of the surface the cause of danger is situated; for, this being determined, a small cautery, corresponding to the opening of the speculum, which includes the open bloodvessel, could readily be made to traverse the isolated portion of the surface, and so to seal the issue of the blood. Or, suppose the case under examination to be one of fistula, the knotty question as to the existence and situation of an internal opening can hereby be cleared up. Search being made in the manner above described, and with the joint advantage of touch and sight, the internal opening, if there be one, can scarcely fail to be detected ; and when discovered, attempt might be made to seal it by the cautery or caustic before having recourse to a serious operation. Thanks to Mr Liston and Dr Pagan, the practicability of effecting the occlusion of even large fistulous openings by such applications is no longer a problem; and there is no need of argument to prove the immediate mitigation of *fistula in ano* which must follow upon shutting off its communication with the interior of the bowel. Thereby the noisome character of the disease would be removed, the constitution would be relieved from the irritation of a foul discharge, and the case be at once converted into a simple abscess capable of going through a mild process of healing. It will be obvious, from an inspection of Fig. 5, that by means of the pinching-screw *l*, the outer prism and its connecting-ring *p* may be applied to any other size of speculum ; and the handle *m*, which is made to unscrew, may be fixed in any of the holes *n* formed in the ring for that purpose, should it be found convenient to alter its position or to transfer it to the hand of an assistant without removing the instrument.

It is unnecessary to multiply illustrations of the simple method of observation explained in this paper, and I would only remark, with reference to the speculum for the uterine passage, that by this way of obtaining observation in the diseases of females, the withdrawn position of the light is calculated to lessen the misery to them attending all professional interference. In such cases, by having a prism appended to the glass aperture of a small lantern, the patient's apartment might be darkened to any desired extent.

It may be, that my confident anticipation of adapting the prism to the examination of the avenues meeting in the throat, have led me to use expressions which to some may savour of hyperbole. If I have exposed myself to such a charge, the inaccuracy is unintentional, and the instruments produced afford ready opportunity of testing the characters ascribed to them. Upon a first examination, the observer will be apt to be satisfied with an indistinct view of objects which he has been used to regard as beyond the reach of observation, and the full power of prismatic illumination will therefore not at once be appreciated. Those who have discovered objects through the different tubes produced only in a shadowy and shrouded light, can have but a very inadequate idea of the much greater satisfaction attainable by the proper adjustment. It will convey a notion of what is meant, when I mention that through a tube of two feet in length and half an inch in bore, I can discern, by a good light, slender initials impressed on red sealing-wax, a test as sufficient as any ever likely to be required for recognising disease in the living body, be it in the stomach itself.

Although in this communication I have confined myself to the consideration of prismatic illumination as applied to the subjects pertaining to my own profession, it will readily occur to the members of this Society, that it may be employed with equal advantage in any department of the arts where light could be used to test the interior condition and soundness of straight narrow cylinders, such as valuable ordnance, &c., and also to some extent of tubes joined together at an angle.

ADAM WARDEN, M.D.

3 BAXTER'S PLACE, EDINBURGH,
12*th April* 1844.

Report of Committee of the Royal Scottish Society of Arts on Dr Warden's employment of the Totally Reflecting Prism for illuminating the open Cavities of the Body, with a view to facilitate the examination of disease, and the application of remedial means in such situations; illustrated with an Ear "Speculum" adapted to this method of observation.

Committee.—Dr Traill, Dr Douglas Maclagan, Dr Cowan, Dr Ransford. Dr Ransford, *Convener.*

The Committee met on Wednesday, 15th instant, and carefully examined this adaptation of the prism; they found, that, in narrow passages, such as the auditory canal, the invention was peculiarly fitted to assist in the discovery of disease; and, from the reports of other medical men, that it was found to be equally satisfactory in examining other cavities of the human body.

Dr Warden also laid before the Committee various surgical instru-

ments, in which the prism may be used with great advantage; and the Committee have no hesitation in asserting, that this is a most ingenious and useful mode of throwing light, either natural or artificial, in all cases in which "Specula" are employed.

The Committee unanimously recommend it to the favourable consideration of the Royal Scottish Society of Arts.

Signed in name of the Committee.

(Signed) CHARLES RANSFORD, M.D., F.R.S.S.A.,
Convener.

EDINBURGH, *May* 22. 1844.

Comparative Analysis of Recent and Fossil Bones.

By J. MIDDLETON, F.G.S.

TO RICHARD PHILIPS, ESQ.

DEAR SIR,—Having lately devoted some time and attention to the analyses of bones, both recent and fossil, I trust some of the results at which I have arrived may not be unacceptable to the readers of the Philosophical Magazine. I took up the subject with the view of ascertaining, if possible, the law by which fluoride of calcium becomes augmented or developed in fossil bones; as, should this be established, an important step would, I conceive, be thereby made towards the ascertainment of geological time.

That fossil bones contain fluoride of calcium in greater quantity than recent ones, is a fact which has long been known, though heretofore not adequately explained. One theory proposed to account for it on the hypothesis, that the source of fluorine in animals is their food, and that in former times it must have contained a greater quantity of the substance than it does now; and thus the bones of animals then living came to possess it in a higher proportion. To this theory, however, I felt unable to consent, as, in order to entitle it to credence, its supporters should, in my opinion, be prepared to shew that fluoride of calcium is capable, from its nature, of occupying the place, and discharging the functions of phosphate of lime in living bones, without detriment to their health and strength; as also, that if a greater quantity of fluoride of calcium were present in the food of animals, a greater proportion would be absorbed. To assume these is to assume too much, the more especially as the general constancy and uniformity of Nature is thereby opposed, who, having her own materials to select from, and to work with from the beginning, is as little likely to deviate in the constituent elements of things, as in the laws by which the things themselves are governed.

It is, perhaps, unnecessary to examine other explanations less generally received, though all equally exceptionable; suffice it to say, that, unsatisfied with them, I was induced to seek for another.

In this search it occurred to me that fluorine might exist in common water; and, if so, that its accumulation in fossil bones would be found to be the result of infiltration, as with carbonate of lime, peroxide of iron, &c. In order to ascertain whether there was any foundation for this view, I examined the following substances, and found them to contain fluorine, some in greater, some in less proportion:—1. Deposit in a chloride of lime vat. 2. Deposit in a water-conduit pipe of a coal-mine. 3. Stalactitic deposit from the old red sandstone.* 4. Deposit in a wooden pipe for conducting water from a building. 5. Deposit in a kettle used solely for the boiling of water. 6. Portion of a vein of sulphuret of barytes from the old red sandstone above mentioned. 7. Fossil wood from Egypt, fossilized by infiltration of carbonate of lime. 8. Fossil wood from Egypt, fossilized by infiltration of silica.†

I no longer entertained a doubt as to the source of fluorine in recent bones, and of its accumulation in fossil bones. The fact which my investigation also disclosed to me, viz. that fluorine is not confined to the bones of recent mammalia, but exists also in those of birds and reptiles, as also in the shells of mollusca, was also thus accounted for. Indeed, the last fact is alone enough to prove that the source of fluorine must be as generally diffused as water is; while the reception of that substance by the organism being secured and placed beyond the dominion of tastes or caprice, seems to elevate in importance the function which it has to fulfil.

I shall now proceed to detail a few of the analyses made in prosecution of the object with which my investigations were begun. My inferences as to the geological bearing of the results have already been submitted to the Geological Society.

First, *Colossochylus atlas* of the Sewalic Hills—entosternal bone of the sternum, Caut. and Falc.; phosphate of lime, 64·95 per cent.; carbonate of lime, 22·36; fluoride of calcium, 11·68; peroxide of iron, 1·00.

Second, Fossil ruminant of the Sewalic—phosphate of lime, 78·00 per cent.; carbonate of lime, 11·34; fluoride of calcium, 10·65; peroxide of iron, trace.

Third, A fossil horse of the Sewalic—phosphate of lime, 58·46 per cent.; fluoride of calcium, 11·24; carbonate of lime, 28·80; peroxide of iron, 0·60.

Fourth, Fossil camel of the Sewalic—phosphate of lime, 62·35 per cent.; carbonate of lime, 25·23; fluoride of calcium, 11·16; peroxide of iron, 0·76.

Fifth,‡ Part of a fossil alligator, Sewalic—phosphate of lime,

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* Contained about 8 per cent. of fluoride of calcium.

† A minute trace of fluorine.

‡ The state of this fossil differed essentially from that of the foregoing; they being soft and friable, hard and refractory, and having quite a mineral character.

75·79 per cent.; carbonate of lime, 7·40; phosphate and peroxide of iron, 8·67; fluoride of calcium, 4·85; carbonate of magnesia, 1·76; silica, 1·50.

The above are analyses of a few of several specimens furnished to me by Dr Falconer, a gentleman whose love of science and perseverance in its cause are only equalled by the cordiality with which he encourages and assists others engaged in scientific pursuits.

Sixth, Iguanodon of the Wealden—phosphate of lime, 35·35 per cent.; carbonate of lime, 19·59; fluoride of calcium, 11·51; insoluble silicates, 8·75; chloride of sodium, 1·26; soda, 2·50; magnesia and chloride of magnesium, 3·50; alumina and peroxide of iron, 6·91; organic, 10·71.

Seventh, Recent shells—carbonate of lime, 99·01 per cent.; chloride of sodium, 0·20; fluoride of calcium,* tissue and loss, 0·79.

Eighth, Sea urchin of the Miocene from Malta—carbonate of lime, 98·12 per cent.; chloride of sodium, 0·48; insoluble silicates, 0·80; fluoride of calcium, 0·55.

For the interesting subjects, of which the following are analyses, I am indebted to the kindness and courtesy of the authorities of University College. It will be readily seen how important they were to the investigations with which I set out, as also how directly they bear upon the truth to which my investigations led me, viz. "that fluorine in fossil bones is a product of infiltration."

The first of these analyses, and the ninth in order, is that of a Greek skull, its age being about 2000 years, as indicated by a coin found under the jaw, and which, according to usage, had no doubt been placed in the mouth of a corpse previous to burial. The bone had so far assumed a fossil character as to be friable; easily pulverized in a mortar, and having a faintly pinkish tint, due to the presence of the peroxide of iron. The following were found to be its constituents:—

Phosphate of lime, 70·01 per cent.; carbonate of lime, 10·34 per cent.; fluoride of calcium, 5·04; organic matter, 9·97; insoluble acids, 1·68; soda and chloride of sodium, 1·15; phosphate of magnesia, 1·34; peroxide of iron a small quantity.

Tenth, Skull of an Egyptian mummy—organic matter, 38·50† per cent.; phosphate of lime, 50·76; carbonate of lime, 6·01; fluoride of calcium, 2·35; phosphate of magnesia, 1·14; soda and chloride of sodium, 1·12.

Eleventh, Analysis of a portion of a skull lately recovered from the wreck of the Royal George. This bone had undergone but little change in appearance from its normal state, while it had all the tenacity of recent bone. It had, however, a slightly yellowish tinge, and the cells between the plates were charged with a white substance which, when examined, was found to consist of chloride and oxide of magnesium. The following were its constituents :—

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* The quantity too small for estimation.

† I can only account for the large proportion of organic matter, and the small proportion of carbonate of lime, by supposing them to be the results of the process of embalming.

Organic matter, 31·59 per cent.; phosphate of lime, 50·58; carbonate of lime, 9·83; fluoride of calcium, 1·86; soda, 1·08; chloride of sodium, 2·42; magnesia and chloride of magnesium, 3·50.

Twelfth, Analysis of a portion of a recent skull—Organic matter, 33·43; phosphate of lime, 51·11; carbonate of lime, 10·31; fluoride of calcium,* 1·99; soda, 1·08; chloride of sodium, 0·60; magnesia and phosphate of magnesia,† 1·67.

It is perhaps unnecessary to add more to these analyses than the statement that they have been performed with great care, and that to these, and congeneric inquiries, I have devoted some months; while pursued as they were in the Laboratory of University College, I had the advantage of most able advice and assistance.‡—I am, &c.

J. Middleton.

London, June 7. 1844.

—*Philosophical Magazine No.* 164, *p.* 14.

On the Occurrence of Fluorine in Recent as well as in Fossil Bones. By Charles Daubeny, M.D., F.R.S.

Having, in the course of the preceding spring, paid a visit to the deposit of compact phosphorite, which occurs in the province of Estremadura in Spain, I was subsequently led to examine into the chemical constitution of the mineral which forms the prevailing ingredient of the vein to which my inquiries had been directed.

The results of my examination have already been reported in the memoir communicated by my fellow-traveller, Captain Waddington, R.N., and myself, to the Geological Society, with respect to the rock in question, and read at their meeting on the 17th of January last; from which it will be seen, that the mineral, although not being crystallized, it is somewhat variable in its composition, yet, when selected as pure as possible, contains as much as 81 per cent. of phospate of lime, and 14 per cent. of fluoride of calcium, the remainder appearing to consist of silica and peroxide of iron.

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* So far as an inference may be drawn from qualitative indications, the bone of a fœtus of 6½ months, contains as great a proportion of fluoride as that of an adult; an interesting fact, and not, I believe, previously noticed.

† If none of the magnesia existed in the bone as phosphate, which there is much reason to doubt, the phosphate of lime would be increased about 1 per cent.; and the fluoride of calcium would be, therefore, proportionally diminished.

‡ The results of MM. Girardin and Preisser's analyses of ancient and fossil bones will be found in *Phil. Mag.* s. 3, vol. xxiv., p. 18.—Ed.

The conclusion arrived at, with respect to the compact form of mineral phosphate of lime occurring in the above locality, coupled with the reports of other chemists to the same effect relative to the crystallized apatite, naturally led me to speculate as to the final causes of the apparently constant association of fluoride of calcium with earthy phosphates, amongst the older materials of the globe, and to ask myself, whether it might not be possible, that fluorine, as well as phosphorous, fulfilled some hitherto unexplained office, in the economy of those organic beings, for the sake of which such mineral matters may be conjectured to have been treasured up in the rock formations from the beginning of time.

These reflections brought to my mind the researches of Morichini and of Berzelius, with respect to the existence of fluorine in bones, seeing that the latter, according to the concurrent testimony of both these philosophers, appear to contain, as a constant ingredient, a minute quantity of fluoride of calcium, inasmuch as its presence is vouched for by them, in recent as well as in fossil bones, and in the teeth of mammalia, as well as in other parts of their osseous structure.

Here, however, I was compelled to pause, by observing the contrary statements put forth by other able chemists relative to this point. Fourcroy and Vauquelin having, previously to the researches of Berzelius, denied the existence of fluorine in recent bones; and Dr Rees having, subsequently to them, in a memoir drawn up under a full knowledge by what had been done before, arrived at a conclusion equally opposed to that of the Swedish philosopher.* One too, which has been since corroborated in a communication relative to the composition of bones, made to the French Institute, by Messrs. Girardin and Preisser of Rouen, and lately published in the *Comptes Rendus*.†

As, however, none of these gentlemen appear to dispute that fluorine does occur *in fossil bones* generally, the conclusion they have arrived at leaves the subject, it must be confessed, encumbered with greater difficulties than before; for as all sound chemical analogies stand opposed to the admission of the idea, that fluorine can have been generated from the other constituents, during any process of decay or alteration that might have occurred in it during the ages that had elapsed since it formed a part of the living structure, we should be driven to the belief, that the fluoride of calcium contained in bone had filtered in from without; a conjecture which, although perfectly plausible, if the concurrence of this ingredient had been casual, or had been limited to bones found in rocks of a certain age or composition, seemed rather a violent one, when extended to those of all ages and formations, being scarcely reconcileable with the rarity of the mineral itself in the waters of springs, and its sparing solubility in most re-agents.

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* See Phil. Mag. S. 3, vol. xv. p. 558. Ed.

† Ibid. S. 3, vol. xxiv. p. 154. Ed.

These difficulties that occurred to my mind, no less than the weight I attached to the positive testimony of the great Swedish chemist, in favour of the existence of fluorine in recent bones, induced me to consider, whether it might not be possible that certain circumstances had operated in the mode of conducting the experiment, by which the presence of fluorine, in the hands of the chemists who adopt the opposite conclusion, escaped detection.

And, on further investigation, it appeared to me, that two ingredients naturally present in recent bones might have interfered with the result in the instances alluded to.

The first of these is animal matter, which, owing to the strong affinity it possesses for fluorine, may arrest its escape, and thus prevent it from coming into contact with the glass; the second, salts containing any volatilizable ingredient, such as the carbonic or muriatic acids, which would be disengaged by the same agent by which the fluorine was set at liberty, and which, escaping in a rapid current, might carry the latter along with them, before it could have time to exert any sensible action upon the glass suspended over it. Accordingly, I found, that whilst one-tenth of a grain of fluor spar, mixed with more than 100 grains of any earthy mineral, occasioned, under the action of sulphuric acid, an easily discernible, though faint corrosion, on the exposed parts of the glass, the same quantity produced no effect whatever, when mixed with 5 per cent. of carbonate of lime, or with a little gelatine; and that half a grain of fluor spar, and the earth, when mixed with gelatine, caused a trace on glass not much more distinct than that occasioned by one-tenth of a grain without this admixture.

In testing, therefore, the bones and teeth which I had obtained for examination, I did not choose to content myself with merely adding sulphuric acid to the pulverized specimen, but I began by burning off all the animal matter; and then, finding that carbonic acid still in part remained, I dissolved the earthy residuum in muriatic acid, and threw down, by means of caustic ammonia, the earthy phosphates.

The latter, after being well washed and dried, were treated with concentrated sulphuric acid in a platina crucible, covered over by a plate of glass, shielded, except on the parts intended to be acted upon, by a coating of wax; but no artificial heat was applied, as the sulphuric acid, by its action upon the phosphate, raised the temperature sufficiently to expel whatever fluoric acid might be present in the specimen.

The glass was allowed to remain as a cover to the platina crucible for at least two hours, and in order to insure the condensation upon it of the hydrofluoric vapour, a rim of wax was placed round the margin of the upper surface of the glass, by means of which a small portion of water might be kept the whole time in contact with it, so as to maintain a suitably low temperature.

That these precautions were not unnecessary, I satisfied myself by observing the difference in the degree of corrosion produced by a fossil bone given me by Dr Buckland from the cave of Kirkdale in Yorkshire, when thus purified from the animal matter by which its long interment had not yet deprived it, as well as of its carbonic acid, as compared with the same when treated with sulphuric acid without having undergone such a preparation.

In proof of this I submit to the inspection of members, specimens No. 3, and No. 4; the one shewing the glass corroded by a Kirkdale bone, deprived of its animal matter and carbonic acid; the latter, by one retaining both. Operating in this manner, I have succeeded in engraving upon glass, not only by means of fossil bones from Stonesfield, from Montmartre, from the cave of Kirkdale in Yorkshire, and from that of Gailenreuth in Franconia, specimens of all which were supplied me by Dr Buckland; but likewise with the bone of some quadruped that had been lying for a long, but unknown time, exposed to the weather in the soil of our Botanic Garden; with the vertebra of an ox recently killed; with the tibia of a human subject from an anatomical cabinet at Oxford; with the teeth of an ox just killed; and with human teeth of recent date. The markings differ widely in the degree of their distinctness, and are, in some instances, so faint as hardly to be discerned except by day-light; but I have convinced myself, that they cannot be attributed to the disengagement of phosphoric acid, as the same glass was in no degree affected by the fumes proceeding from the action of sulphuric acid upon pure phosphate of lime, where the acid had been derived from the direct combustion of phosphorous, nor, for a long time at least, by the vapour of free phosphoric acid exposed to a heat sufficiently great to fuse and partially to volatilize it.

Nor was it dependent on any peculiarity in the nature of the glass, for plate glass was corroded in the same manner as the crown glass, more usually employed.

By the oldest and most fossilized specimens, the glass seemed undoubtedly to be the most deeply etched; yet even here there occurred exceptions, for the marks caused by a bear's bone taken from Gailenreuth, are the faintest in the whole series, and were produced only after a long exposure to the acid vapours, two trials having proved unsuccessful; whilst, on the other hand, the tibia of a human subject gave indications almost as distinct as any of the fossil bones operated upon.

It would doubtless have been more satisfactory if I could have stated the proportion of fluorine in these samples of bones and teeth, as well as the fact of its actual presence, and likewise if I had extended my examination over a larger number of specimens; but I have been compelled to postpone the former part of the inquiry until I could obtain an apparatus suitable for the purpose, and doubted when my time would permit me to carry further the present investigation, if, in order to give my results in a state of greater completeness, I neglected the present opportunity of communicating them.* The only criterion, therefore, I am at present enabled to offer as to the proportion of fluorine in the bones examined, is a comparison of the depth and distinctness of the marks produced by the latter, with those caused by a certain amount of fluor spar, mixed with a weight of phosphate of lime, or other earthy material, equal to that present in the bones operated upon. Judging by this rough mode of measurement, it would appear, that in several instances the faintness of the marks shews a smaller quantity of fluorine to have been present in the specimen, than would have been contained in a mixture of one-tenth of a grain of fluor spar added to 100 grains of phosphate of lime.

The existence of fluoric acid, as a constant, or at least a common ingredient in bones of all ages, would seem, *a priori*, to be much more probable than its absence in recent bones would be, if its normal presence in fossil ones be admitted, for we can readily understand its finding its way into the animal structure through the medium of plants, which may imbibe it along with those phosphates with which it is so generally associated. Indeed it seems so likely, that those vegetables at least that contain much phosphate of lime should possess a trace of it, that I am at this very time examining the ashes of barley with reference to the latter point.†

The greater distinctness of the marks produced by the fossil bones acted upon than by the recent ones, may be more difficult of explanation; but before it is urged as an objection against the view taken, it should be determined whether the difference may not arise from the removal of the greater part of the animal matter from the fossil bone, owing to its long interment in the earth. Of the six specimens of fossil and recent bones of which I made a rough analysis, that from Stonesfield, which was the oldest of any, having been imbedded in a secondary rock belonging to the oolite formation, lost, by exposure to a heat of 212°, 4·2 per cent.; by a further heat of about 500°, 5·0 per cent. more, and by increasing the temperature to a red heat, only 1-8 per cent. in addition, the latter probably representing very nearly the amount of animal matter remaining, the two former numbers the water retained within the bone.

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* I have since, by the aid of the apparatus described in the former note, attempted to estimate the amount of fluorine in the fossil bone from Stonesfield, and in the recent human bone from an anatomical cabinet. The former afforded 8·7 grains per cent. of fluoride of calcium, the latter only 2·0, results which will at least indicate the *relative*, if not the *absolute* quantity of fluorine present.

† I have since ascertained that no sensible action is exerted on glass by heating with sulphuric acid the earthy phosphates present in 12 lbs. of barley. Sprengel, I find, had already suggested the probable occurrence of fluorine in plants, but conceives that it exists in such a state of combination, as causes it to be dissipated by the heat necessary for expelling the carbonaceous matter, and therefore cannot be detected in the ordinary method.

Proceeding upon the same data, the bone from tertiary rocks of the Paris basin, next in the order of antiquity, would contain 10 per cent. of water, 2 of animal matter; the bone from Gailenreuth, water 13.9, animal matter 5.0; that from Kirkdale 12.5 water, 11 animal matter; whereas the recent bone picked up in the Botanic Garden contained, even when dry externally, about 30 per cent. of water, and 11 animal matter; and the human tibia, which had been kept in an anatomical cabinet for a certain time, gave out 23 per cent. of water, and 17 of animal matter.

It may also be suggested, as a possible explanation, that the fluoride of calcium distributed through the mass has, in the course of time, become collected into little nuclei in certain parts of the bone, and for this reason may allow of a more ready disengagement from it of the fluoric acid which it contains, as an ingredient.

That a certain alteration in the arrangement of the earthy particles of a bone does occasionally take place after its deposition, is evinced from the curious observations, by Messrs Girardin and Preisser, in the memoir which has been already referred to, as these gentlemen state, that the bone-earth phosphate appears, in some instances, to have separated into two distinct compounds, crystals of apatite being recognised by them in some of the fossil bones in their possession, which they conceive to have arisen from the segregation of the tribasic from the bibasic compound.

Will not this latter fact also help us towards an understanding of the function which fluate of lime may fulfil in the structure of bones, and likewise of the peculiar adaptation of the bone-earth phosphate to serve as its prevailing earthy ingredient?

It seems a general law in both kingdoms of organic nature, that crystallization should operate as a sort of antagonist force to the process of assimilation, so that no material can be fitted to enter into the fabric of a living body, between whose particles the natural force of polarity operates with all its energy. Hence, according to Dr Prout, the use of the infinitesimal small portions of foreign inorganic matter interposed between the particles of most bodies, which form the constituents of vegetable or animal organization; and although it may be true, as has been suggested by Von Buch, that the very prismatic form which belongs to the phosphate of lime as a mineral species, adapts it for the fibrous structure of bone better than other earthy compounds, in which the axis of crystallization is equal in both directions, yet, even in this case, the tendency to arrange itself according to the laws which regulate inert matter might operate too powerfully, were it not diminished by the association, in equal atomic weights of the two phosphates, each of which possesses a polarity in some degree differing from the other, and consequently, to a certain extent, counteracts the disposition in the particles of the other to assume a determinate arrangement.

If there be any truth in these speculations, is it not also conceivable, that the interposition of a mineral matter, like fluor-spar, whose particles crystallize in quite another manner, that is in cubes, may co-operate on the same principle, in imparting that freedom of motion to the particles of the prevailing constitutent of bones, by which it is rendered more pliant to the purposes of the animal economy, more obedient to the laws of life, more ready, in short, to insinuate itself into the pores, so as to form the coats of those delicate capillary canals, of which the osseous structure appears to consist?—*Philosophical Magazine*, vol. xxv. No. 164, p. 122.

On the Existence of Phosphoric Acid in Rocks of Igneous Origin. By GEORGE FOWNES, Ph. D., Chemical Lecturer in the Middlesex Hospital Medical School. Communicated by THOMAS GRAHAM, Esq., F.R.S., &c.*

The important, though obscure, functions attributed to the elementary body, phosphorus, both in the vegetable and in the animal kingdoms; and the well known fact, that rocks of nearly every description afford, on disintegration, soils more or less capable of supporting the life of plants, and from which, consequently, phosphoric acid cannot possibly be absent, seemed to render a search for that substance, in rocks of igneous origin, generally very desirable, because if there found, an easy and satisfactory explanation of the origin and first source of the element in question would be given. As I am not aware that any direct researches on this subject have yet been made, or at least placed on record, I venture to submit to the notice of the Royal Society, the results of a few experiments made by myself, which, so far as they go, resolve the question in the affirmative.

The first substance tried was the fine white porcelain-clay of Dartmoor, Devon, the result of the disintegration of the felspar of the granite of that district. This is one of the chief components of porcelain, and of the finer kinds of English earthenware, and was found on analysis to correspond very closely in composition with that of the material employed in the manufacture of the Sevres porcelain.

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* Phil. Trans. Royal Soc. London. Year 1844. Part I., page 53.

It was thought that phosphoric acid, if present, would be in combination with a portion of the alumina; and as the phosphate of that earth is readily soluble in dilute mineral acids, while the silicate offers great resistance to these agents, mere digestion with acid would suffice to extract the whole, or the greater part of the phosphate, which could be afterwards precipitated by an alkali, and examined.

With this view, 1000 grains of the clay were boiled during several hours in a flask with a quantity of pure dilute hydrochloric acid; a large bulk of distilled water was then added, and the whole allowed to rest until perfectly clear. The acid liquid was then carefully decanted from the undissolved clay, evaporated in a porcelain basin to a small bulk, and precipitated by a slight excess of pure ammonia. The scanty reddish precipitate obtained, which consisted chiefly of alumina and oxide of iron, was collected upon a little filter, thoroughly washed with distilled water, dried, and ignited. It was next reduced to fine powder, and mixed with an equal weight of pure silica in a finely divided state, and six times as much anhydrous carbonate of soda. This mixture was heated to fusion in a platinum crucible. When cold, the melted mass was acted upon by boiling water, and the soluble and highly alkaline portion separated by a filter from the insoluble silicate of alumina. The solution was mixed with excess of nitric acid, evaporated to dryness, water added, and the product filtered. The liquid thus obtained was divided into two portions; one of these was carefully neutralized by a little ammonia, and mixed with a few drops of nitrate of silver; a distinct yellow precipitate appeared which was freely soluble in dilute nitric and acetic acids. The nitric acid was mixed with excess of ammonia and some hydrochlorate of ammonia, and a few drops of solution of sulphate of magnesia added. After a short interval, a crystalline, granular, white precipitate, the ammonio-magnesian phosphate, made its appearance, which increased in quantity by agitation.

This experiment, which demonstrates the presence of a small quantity of phosphoric acid in the clay in a most unequivocal manner, was several times repeated with a like result. The purity of the acids, carbonate of soda, and other materials employed, were rigorously tested, and filtration through paper of the original acid' liquid purposely avoided, lest a trace of earthy phosphate should have been dissolved from the paper.

The porcelain-clay is extracted from the disintegrated granite by mere washing with water, and subsidence; and the water of the district in which it is found, is, in all probability, exceedingly pure. It was thought worth while, however, to examine in the same manner the decomposed rock which had not been subjected to any artificial treatment, and a specimen taken by myself from the quarry was chosen for the purpose. The result shewed the presence of phosphoric acid as in the clay, and apparently to about the same extent, allowance being made for the quartz-grains, mica, &c.

In the examination of unaltered felspar, I failed, unfortunately, in getting a conclusive result. The mineral, although reduced to very fine powder by trituration in a mortar of Swedish porphyry, was found to be so hard and dense as to resist completely the action of the acid at a boiling temperature. An insignificant quantity of oxide of iron was dissolved out, in which no phosphoric acid could be detected. 200 grains of the powdered felspar were then fused with a large excess of carbonate of soda; the mass was treated with water, filtered, the solution supersaturated with nitric acid, and evaporated to dryness; water was then poured upon the residue, and the whole placed upon a filter. The solution was then examined, as before, for phosphoric acid, but with an indistinct and doubtful result. Too small a quantity of the felspar had been used, and the mass of nitrate of soda present interfered too seriously with the action of the tests to render their evidence of any value. A far better mode of investigation would be, to act upon the powdered mineral with hydrofluoric acid, in the manner recommended by some analysts in the examination of natural silicates containing an alkali; not being, however, in possession of the necessary platinum vessels, I was obliged to abandon the attempt.

Other substances were then tried with very decisive results. The method of proceeding adopted was very much the same as that already described. The minerals were finely powdered in the porphyry mortar, and boiled, as before, with dilute hydrochloric acid. All were much more readily attacked than the porcelain clay, and yielded solutions containing a large quantity of alumina and oxide of iron. The liquid was separated from the insoluble part by decantation, evaporated nearly to dryness, water added, and then an excess of ammonia. The copious bulky precipitates obtained were washed and digested in dilute acetic acid, which has the property of dissolving, with great facility, both oxide of iron and alumina, while it leaves untouched the phosphates of those bases. The undissolved residue was dried, ignited, fused with silica and carbonate of soda, and the product examined in the manner already described. The addition of silica is indispensable to the retention by the whole of the alumina in an insoluble condition. Phosphate of alumina is not decomposed by carbonate of soda by fusion, or only partially, and is, besides, soluble in an aqueous solution of that salt.

The results of the examination may be thus briefly stated:—*Dark grey vesicular lava from the Rhine, used at Cologne as a building stone, being exceedingly strong and durable.*—Enough of phosphate of soda was extracted from 1000 grains of this substance to exhibit the yellow phosphate of silver, and the phosphate of magnesia and ammonia on a large scale. The phosphoric acid might be said to be very *abundant*, that is, comparatively speaking. No attempt was, however, made to estimate it quantitatively, as the operation is attended with great difficulty, and the result of doubtful value, from the unavoidable errors of experiment bearing too large a proportion to the quantity of the substance.

White trachyte of the Drachenfels, near Bonn, on the Rhine.—This rock is apparently as rich in phosphoric acid as the preceding; nothing could be more distinct and satisfactory than the indications of the re-agents.

Dark red, spongy, scoriaceous lava from Vesuvius.—This was tried in the same manner, and yielded abundance of phosphoric acid.

Compact dark green *basalt,* or toadstone, from Cavedale, Derbyshire.—This substance was very tough, and difficult to powder. Enough of phosphate of soda was, however, extracted from 750 grains of the rock, to exhibit very unequivocally the characteristic tests described.

Dark blackish-green, extremely strong basalt from the neighbourhood of Dudley, termed *Rowley-ragg*, gave a very similar result. Phosphoric acid is not so plentiful in these substances as in the lava, although its presence is easily rendered evident.

An ancient phosphyritic lava, containing numerous crystals of hornblende from Vesuvius.—This phosphoric acid was here very distinct, but not so abundant as in the more recent lava.

A specimen of tufa, or *volcanic mud,* also from Vesuvius, was found to contain phosphoric acid in notable quantity.

These were all the substances tried; they were taken, as is at once seen, indiscriminately from igneous formations of many localities and many ages, and they all, with one doubtful exception, in which practical difficulties interfered with the inquiry, yielded phosphoric acid. It is highly probable, therefore, that this substance is a very usual, although small, component of volcanic rocks.

It is not unlikely that the remarkable fertility possessed by soils, derived from the decomposition of some varieties of lava, may be, *in part at least*, due to the presence of this phosphate in the original rock, although much must of course be ascribed to the alkali, especially potash, which these substances contain, and which is gradually brought by the continued process of disintegration into a soluble state. There can be little doubt that the matter erupted from time to time from the interior of the earth, in a state of fusion, is thus destined to renew the surface from which the more valuable and more soluble components have gradually been removed by the action of water and other causes constantly in operation. If it should hereafter be found, on a more extended investigation, that phosphoric acid, although present in all igneous rocks, is most abundant in those of modern date, the fact will thus receive an explanation, the more ancient lavas having been most changed by the slowly-acting and almost imperceptible causes in question. One might be attempted to consider lava as a kind of fundamental material, from the subsequent alteration of which all others are derived, and expect it to contain, here and there at least, traces of all the elementary bodies known, even those most rare. In the present case, it cannot be altogether devoid of interest to trace to its first source the enormous quantities of phosphoric acid, for the most part locked up in a temporarily insoluble condition in the vegetable and animal kingdoms, and in the various strata of calcareous and sedimentary deposits, in the formation of which, organized beings have played so prominent and important a part.

An Account of Electrical Experiments. By Mr R. ADIE, Liverpool. Communicated by the Author.

Among the details of the experiments as originally drawn up by me for publication, in the 70th and 71st Numbers of Jameson's Philosophical Journal, I had some which shewed a molecular action in the joint of M. Pelletier's cross, when subjected to the continued influence of a feeble electrical current for six weeks. Ultimately, these experiments were withdrawn from the first series submitted for the consideration of your readers; for I failed in an attempt to detect any action on the galvanometer, from the slow mechanical fracture of a thermo joint, and the disappointment in this experiment made me desirous of examining more at leisure the details of the subject.

The results I have now to offer, appear to me to lead by steps to the explanation of the sources of the electrical currents noticed by M. Pelletier; but I regret that they do not confirm an observation deduced from his original experiments, namely, that a current of electricity, in passing from one metal into another, can, under some circumstances, lower the temperature of the joint below that of the surrounding atmosphere; or, in other words, can produce cold. It will be seen that a given current of electricity may heat a joint 12°, by passing through it in one direction, and only 2° when passed in an opposite direction; but in no instance is the temperature ever reduced below that of the apartment where the investigations are carried on. Another result which these experiments go to prove, and to my mind the more important of the two, is, that an electrical current, in passing across any medium, heats the part where it enters higher than the part where it quits the medium.

35. To the extremities of a galvanic battery of ten pairs of zinc and silver plates, superficies of each plate in action about 50 inches, two small platina capsules were attached by bands or ribbons of copper, so that these capsules could be used as a pair of decomposing poles. They were then inserted to about two-thirds of their depth, in acidulated water, with nearly a superficial inch of surface in action as a decomposing pole. On completing the circuit, a brisk decomposition commenced, which kept the platina surfaces well covered with gas bells. Inside the capsules, I had two delicate thermometers,

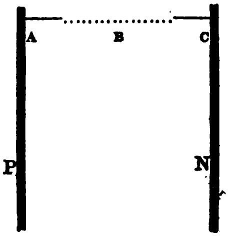
with a small quantity of water, sufficient to cover their bulbs. Both thermometers soon indicated an elevation of temperature, the one in the positive capsule standing the highest; after 15′ action, the observed temperatures were,—apartment, 42°; negative pole, 59°; positive pole, 62°; in this case, the capsules were 2½ inches, as under; on approaching them together, till nearly in contact, the rapidity of the decomposition increased; and after a similar interval of 15′, the negative pole was 62°; the positive, 66°. The temperatures stated were verified by counter-experiments, with the poles reversed, to guard against any error which might arise from inequality in the capsules, or in the extent of their surfaces in action.

The next experiment was made with the same battery, and it was designed to ascertain if the double volumn of hydrogen evolved at the negative pole, could occasion the reduction in temperature there noticed. The platina capsules were replaced by two larger copper ones, superficies in action 3½ inches; these were made to decompose in a saturated sulphate of copper solution, where there is no gas given off; the thermometers were arranged as for the first results. The temperatures observed, after an interval of 15′, were, for the room, and a large vessel containing a solution of sulphate of copper, 46°; negative pole, 46°; positive pole, 47°; the small bulb of a thermometer placed betwixt the two capsules 48°; counter-experiments, like those for the first set, were made, to verify these small differences of temperature. The extent of the changes could not be expected to approach those given for the platina capsules, where the surface in action was 3½ times less, with the resistance to conduction greater.

36. A piece of platina wire, 1½ inches long, was connected by two stout copper wires, with the poles of a five pair galvanic battery, which I found sufficient to keep the temperature of the platina at a full red heat; in this common experiment there is always a portion of the thin wire, where it joins the stout copper wire, which remains dark, the present operation was arranged, to ascertain if the dark portions on both sides were equal; to prevent the interference of currents of air, a glass shade was used to cover the whole.

P, fig. 1, is the stout wire from the positive pole; N, the negative wire; A, the dark portion of the platina wire on the positive side, which always appeared shorter than the dark portion C, on the negative side; B, the incandescent part of the wire. I reversed the poles and repeated this experiment several times, always with the same result. To me it appears to be the readiest proof of the difference of temperature betwixt the extremities of an electrified wire; but, as it is impossible to give measurements of the relative lengths of the dark parts A and C, I removed the thin wire

Fig. 1.



A C, and in lieu placed a bar of bismuth, 8 inches long, and .2 of an inch square. The naked bulbs of two small thermometers were put, one near each end, touching the upper surface of the bismuth, and fastened down to the bar by a single fold of thick soft cloth tied round with thread; this effectually prevented the exposure of any part of the bulbs to the air, while the coverings of both were as nearly as possible equal. I may here mention that I have found this method of applying thermometers to test the temperatures of different parts of electrified bars, worthy of every reliance; for, in no instance, through a number of trials, has the result of the reversed poles ever contradicted the first differences in temperature shewn. The bismuth bar, with its attached thermometers, was allowed half an hour to settle in temperature, then it was connected to a battery consisting of a single pair of zinc and silver plates, superficies in action 42 inches; after an interval of 30′, the observed temperatures were,—room, 46°; positive end, corresponding to A, fig. 1, 59°; negative end, 54°. Another trial, room, 50°; positive end, 60°; negative end, 55°; shewing a difference of 5° betwixt the two extremities of a bismuth bar, when conducting the electricity from a single pair of plates.

A still more marked effect, due to an electrical current, raising the temperature higher at the part where it enters a medium, is given by Professor Daniell. "There is another well established and remarkable effect of the heating power of the voltaic current, which is as yet unexplained. When the conducting wires from a powerful battery, cross one another, and are brought in contact, upon separating them to a short distance, a flame will appear between the two, and the zincode (the positive pole) will become red-hot, and that connected with the generating metal (the negative pole) will remain dark, and comparatively cool. This effect is constant, of whatever metal the conducting wires may be made."* The experiment quoted, appears to me to differ only in degree from those given for fine platina wire, and a bar of bismuth; a powerful current of electricity has to pass through a short space of air; where the resistance to conduction is great, the side where it enters is highly heated, while the side where it escapes from the bad conductor remains cool.

37. For the foregoing experiments, the wires which carried the fluid from the battery to the bars of metal, where the difference of temperature were examined, offered less resistance to the passage of the electricity than those bars. I, therefore, wished to try the effect of conducting wires, which would offer a greater resistance to conduction than the bars tested.

Iron wires were attached to a single pair battery, superficies of each plate in action 42 inches, and an 8-inch copper bar 2 of an inch square, with 2 attached thermometers, was arranged in every way the same as in the experiment with the bismuth bar. The observed temperatures were, room 49°; positive end 50½°; negative 50.° The same experiment repeated with a 2 pair battery—room 45°; positive end 49°; negative end 48°.

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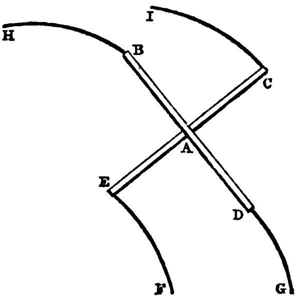
* See Daniell's Introduction to Chemical Philosophy, page 472.

The iron conducting wires were changed for lead ones, and the current derived from 1 pair passed through the copper as before; the observed temperatures were, room 49°; positive end 55°; negative end 56°, In these experiments, the temperatures of the conducting wires and bars must act and re-act on one another through their metallic contact. In the iron wire the resistance to conduction is not sufficient entirely to counteract the natural action of the electricity on the bar of copper; but with lead conducting wires, where the resistance is increased, the negative end is 1° warmer than the positive; at the negative end the electrical current has to enter lead, which it heats considerably, on account of its bad conducting power, the lead re-acts on the copper, and produces this apparent elevation of temperature at the negative pole of the copper bar, which is due to the electrical current entering into a lead bar, or acting on it as a positive pole. With bismuth conductors, the change would, I apprehend, be still greater.

In these experiments, electricity passes through matter in its three different conditions of gaseous, fluid, and solid, and in all of them *heats the part where it enters higher than the part where it quits the medium.*

38. Figure 2 represents one of M. Pelletier's crosses. BD, a bar of antimony, 8 inches long, .2 square; C E, a similar bar of bismuth fastened together at their centres A, either by soldering, or by cleaning the surfaces of the bars where they are in contact and binding them firmly together by cord; H B and I C are copper wires soldered to the bars at band C, for the purpose of conveying a current of electricity from a battery; E F and D G, are similar wires joined for connecting with a galvanometer.

Fig. 2.



When a current is circulated through the joint A in the direction B A C, a galvanometer attached to the extremities of the wires F and G is deflected, as if the joint A was heated; but when the current is circulated in the direction, C A B, the action on the galvanometer is the same as if a piece of ice had been placed on A. This result is constant for all electrical currents, at least within the limits of intensity given by a thermo-battery, and a 10 cell zinc and silver battery. To shew the connection of these currents which act on the galvanometer, with the variations in temperature produced by the primary current in its passage along C A B. I made the following experiments.

Two bars of bismuth and antimony, 8 inches long each, were tied together in the form of letter V, and ribbons of copper fixed to their upper ends for connecting with a battery,—the joint of the bars was placed in the bottom of a conical wine-glass, beside a delicate thermometer, and a small quantity of water, sufficient to cover the thermometer bulb and the joint. The heating power of a hydro current, derived from a single pair used in experiments (37), was now tested by observing the change of temperature of the water. The duration of each trial was 30′, and an interval of 30′ intervened betwixt every experiment, to allow the temperature of the parts to settle. Then the antimony was replaced by a similar bar of copper, and another set of results taken. Again lead was substituted for the copper, and the heating effects similarly tested. The observed results were—

Battery current, passing from the thermo positive to the thermo negative metal.	Battery current, passing from the thermo negative to the thermo positive metal.
Bismuth and antimony couple gave 3° increase in temperature.	Gave 9° increase in temperature.
……………… copper ………………… 2 …………	……. 5 ………………………………
……………… lead…………………… . 0½ …………	……. 2 ………………………………

The bars used in the foregoing experiments were now tied up in the form fig. 2; the bismuth bar being connected with each of the other 3 in the same order as above. A feeble current of electricity derived from a pair of plates excited by water only, was then passed through B A C, and the action on the galvanometer attached to F and G noted. The deflections were not very regular in their extent; to compensate for this, I took the mean of a number of experiments, omitting the fractional parts as unnecessary, bismuth and antimony gave 48°
…… … copper … 16
…… … lead … 8

Here the action on the galvanometer corresponds with the changes of temperature produced by similar joints immersed in water, from which I infer, that the currents in M. Pelletier's cross are the results of differences of temperature only; and, in a variety of experiments, made with thermo joints, I have never seen any evidence of a reduction of temperature or cold. The arrangement which produced the widest difference in temperature of a joint heated by a current, passing, first, in one direction, then in another, was, when the bismuth and antimony couple, used to heat water in a wine-glass, were arranged with a piece of the bar of bismuth projecting beyond the joint into the water, which did not touch the bar of antimony. The current passing from the thermo positive metal increased the temperature 2°, the contrary current 12°.

39. The experiments I have now submitted, appear to me to offer

an explanation of the apparent reduction of temperature, caused by an electrical current passing through the cross, fig. 2, in the direction C A B. The bismuth bar is unequally heated (36), the temperature of the positive, or entering end, being the highest, which developes a thermo-electrical current, passing in the direction A C I, or left to right; but the fluid in the cells of the battery, connected with the wires H and I, or the resistance in a powerful thermo electrical scource, prevent the current passing in that direction, *it finds less resistance to its passage through* A E F, *where the direction is unchanged;* and it acts on the galvanometer, as if a piece of ice had been applied at A, while this joint is in reality slightly heated. Before leaving the consideration of M. Pelletier's cross, I feel bound to state, that should men of science attach any value to the whole of the series of experiments now placed before them, it was the action of the electrical currents on this cross which first occasioned me to commence these inquiries.

40. The elevation of temperature, where an electrical current passes from a good conductor, copper wire, into an inferior one, lead, supplies a ready mode of watching the action of a constant battery. For this purpose, the bulb of a thermometer has to be placed upon the joint, and wrapped round with a good non-conductor of heat; the height the thermometer stands above the temperature of the room indicates the activity of the battery.

The annexed fig. 3, is a representation of this calorific galvanometer. A A, a thermometer.

B, a bar of metal, a good conductor of electricty.

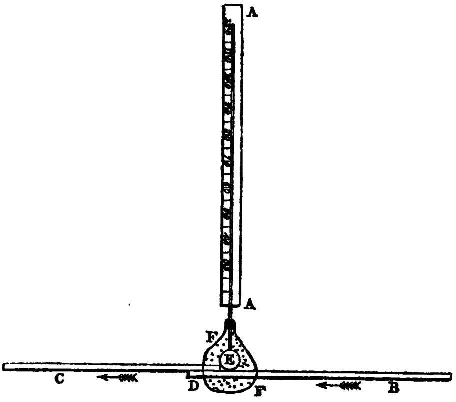
C, a similar bar of metal, a bad conductor.

D, the joint of the two bars, where they are either soldered, or tied together.

E, the thermometer bulb placed in contact with the bar D C.

F F, an envelope for the bulb E, made of cotton wool. The two arrows indicate the direction of the electrical current.

Fig. 3.



When the electricity, from a newly prepared Smee's battery of 48 superficial inches surface, was passed along a pair of antimony and bismuth bars, arranged as above, the thermometer A A rose 19° above the temperature of the room. This amount of action is not long sustained in Smee's battery, the galvanometer indicated only 9° at the end of the first 48 hours, then the decrease in the electrical current is much more slow; and it required 8 days' continued action for the galvanometer to fall through the remaining 9°.

With joints disposed like fig. 3, I made a number of experiments to try the effect of electrical currents on them; but as all of the results obtained could be accounted for, by the influence of the heat which electricity developes in passing across those joints, it seems to me to be unnecessary to enter into the details of the experiments in question.

41. There are some remarks on voltaic phenomena by Professor Grove and Mr Mackerell,* which may be thought at variance with the fact of the unequal heating of any medium traversed by an electrical current, as I have above attempted to prove. For Professor Grove has shewn that in rapid decompositions, a more brilliant combustion is observed at the negative pole than at the positive; while in my experiments the temperature of the negative pole is always the lowest. This apparent contradiction can be reconciled, when the necessary conditions for the appearance of combustion at the poles of a battery are examined. I have always found that the decomposing surface must be enveloped in gas when the spark appears; it is while the electricity is crossing this gaseous atmosphere that combustion takes place (see 1st series 26.) A consequence of this is, that where the largest volume of gas is evolved from a given superficies, there the sparks should be the most brilliant. Where acidulated water is decomposed, there is double the volume of gas eliminated at the negative pole, compared with the gas at the positive pole; hence the greater brilliance in Professor Grove's experiments, when the voltaic circuit is completed by dipping the negative wire; for the same surface of wire has to evolve twice the volume of gas which it had to do when the contact was made by dipping the positive wire into the acid solution. By employing a positive wire of half the section of the negative wire, the difference noticed disappears. The crackling noise stated by Professor Grove to accompany these rapid decompositions, becomes much sharper when sulphuric acid S. G. 1850, is substituted for acidulated water. It appears to me to be occasioned by the rapid formation of gas bells, exactly like steam bells formed in pure water boiling in a smooth glass vessel. The noise in both cases is very similar, and with sheathed poles the bells of gas may be seen to quit the pole at each sharp sound heard.†

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* See Elec. Mag. pp. 121 and 277.

† The experiments with platina and copper capsules detailed above, were performed during last winter, when the ground was partially covered with melting snow. The temperature of that season was unfavourable for secondary decompositions, and as I wished to obtain greater changes in the temperatures with copper poles decomposing sulphate of copper, I returned to them in midsummer, and employed small capsules of ½ a su-

On the Origin of the Nilotic or Egyptian Population. By SAMUEL GEORGE MORTON, M.D.

Since the physical characteristics of the ancient Nilotic population, as derived from history and the monuments, coincide, in a remarkable manner, with the facts derived from anatomical comparison, it becomes necessary to offer some explanation of these results; or, to shew at what period, and under what circumstances, several different branches of the Caucasian race were blended into a single nation, possessing more or less the chracteristics of each, and this, again, modified in degree by another race wholly different from either. It is, in the first place, necessary to recur to the fact of the very long occupation of Egypt by successive dynasties of Hykshos, or Shepherd Kings, and that these were not of one but of several nations—Phœnicians, Pelasgi, and Scythians; while to these followed, at a long interval, an Ethiopian or Austral-Egyptian dynasty. Each of these great revolutions must have tended, in turn, to the amalgamation of the Egyptians with other nations; and this result may be referred to three principal epochs, independently of several subordinate ones.

The *first epoch* embraces the dynasty of the Hykshos or Shepherd Kings, commencing before Christ two thousand and eighty, and having a duration of two hundred and sixty years.

It is important, however, to observe, that Josephus, quoting Manetho, makes the Hykshos dynasty last five hundred and

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perficial inch of surface. The electrical current derived from 2 cells of Daniell's battery, 1 quart each, gave the observed temperature, room 68°, negative pole 71°, positive pole 73½°. The same battery acting on silver capsules decomposing a salt of silver, shewed changes in temperature of 2 less. When small quantities of the fluids were used in the voltameter, I found the temperatures before beginning the electrical part of the experiments frequently 2° below that of the surrounding air. This was evidently a hygrometric effect, but the two sources of change of temperature, namely the unequal heating effect of an electrical current and rapid evaporation when acting together, might very readily be mistaken for the development of cold by electricity.

eleven years; and the learned Baron Bunsen, whose work has not yet appeared, extends it to one thousand, beginning B. C. 2514.* The shorter period is that of Rosellini; but the longer one is, perhaps, most consistent with facts, and at least makes room for those various dominations which, in the lists of Manetho, precede the eighteenth dynasty; which last, headed by Amrenoph the First, drove out the intrusive kings. During this long period the legitimate sovereigns were exiled into Ethiopia; and it is evident that, had Meroë been any other than a province or dependency of Egypt, it is hardly probable that the Egyptians—kings, priests, and people—could have found a safe asylum in that country during the long period of their exile. It is expressly stated by Josephus, that the Shepherd Kings lived at Memphis, "and made both the upper and lower country pay tribute." It would appear, however, that, during the greater part of the Hykshos dynasty, the Egyptians retained possession of the Thebaid; nevertheless, the occupation of Lower Egypt by their enemies, must have effectually precluded all communication with other countries excepting Ethiopia, Southern Arabia, and India; which fact will account for a vast influx of population from those countries, (and, consequently, from the slave regions of Africa,) into the Upper Nilotic provinces.

It is, moreover, reasonable to suppose that, even after the expulsion of the Hykshos, multitudes of Egyptians would remain in Ethiopia,—that country wherein whole generations of their ancestors had lived and died; at the same time that great numbers of Meröites, influenced by a variety of motives, and especially by social alliances, would descend the Nile into Egypt.

It is, moreover, evident, that while the Egyptians became thus fraternized with the nations of Southern Asia, and the motley races of the Upper Nile, the provinces of Lower Egypt would be overrun with the Caucasian tribes of Europe and Western Asia; for these, either as cognate with the Hykshos, or as allies in their service, must have been in immense number to have conquered so populous a country, and especially to have kept possession during so long a period. It is to these events, then, that we attribute that blending of nations which appears to have been coeval with the early ages of the Nilotic family, and which amply accounts for the ethnographic diversities everywhere manifest on the monuments.

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* See Mrs Hamilton Gray's History of Etruria, vol. i., p. 29.

The *second epoch* is comprised in the Ethiopian dynasty of three kings, which lasted forty-four years, beginning B.C. 719.

These Meröite or Austral-Egyptian kings, during their intrusive occupation of Egypt, would naturally, and indeed necessarily, engage the neighbouring tribes, and especially such as were hostile to Egypt, as mercenary soldiers; and there are more than conjectural grounds for believing that the negroes themselves were thus employed. We are told in the Sacred Writings, (2 Chron., chap. xii.,) that when Shishak king of Egypt —who is identical with Sheshonk of the monuments—went up against Jerusalem, he took with him "twelve hundred chariots and three score thousand horsemen: and the people were without number that came with him out of Egypt; the Lubims, the Sukkiims, and the Ethiopians." Of this multitude we may presume that the horsemen, and people in chariots, were part of the Egyptian army; the Lubims and Sukkiims are by most commentators regarded as Libyans and Meröites, while, as the Ethiopians are placed last on the list, and are designated, in the Hebrew original, by the name of *Cush*, it is not unreasonable to suppose that they were Negroes. This view is sustained by a passage in Herodotus,* who states, that in the army of Xerxes which invaded Greece was a legion of *Western Ethiopians*, "who had hair more crisp and curling than any other men." † Now, if the army of Xerxes embraced a legion of African negroes, it would not be remarkable if the Egyptian troops should have been composed in part of the same people; which, indeed, with respect to the Ethiopian dynasty, may be assumed as a thing of course; for the Meröites would naturally avail themselves of every expedient to establish their power by augmenting the number of their exotic confederates, and by extending to them those privileges which had once been sacred to particular castes. For these and other oppressive acts, the Meröite kings were hated by the Egyptians; and no sooner were they expelled than their names were erased from the monuments.*

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* In my *Crania Americana*, note, p. 29, I have employed this passage to shew, that those *Colchians* whom Herodotus mentions as forming "part of the troops of Sesostris," might have been Negroes acting as mercenary or auxiliary soldiers. I am now satisfied that such explanation is at least unnecessary; and I, therefore, take this occasion to withdraw it.

† Polhym., cap. lxx.

The *third epoch* dates from the conquest by Cambyses, B.C. 525, and continues through the whole of the Persian dynasty, or in other words, until the Ptolemaic era, B.C. 332,—a period of nearly two hundred years.

Every one knows that the Persian dominion in Egypt was marked by an utter disregard of all the established institutions. No occasion was omitted which could humble the pride or debase the character of the people. The varied inhabitants of Europe, Asia, and Nigritia, poured into the valley of the Nile, abolishing in degree the exclusiveness of caste, and involving an endless confusion of races.

The prelude to these changes and misfortunes can be traced to the reign of Psammeticus the First, who permitted to foreigners, and especially to the Greeks, a freedom of ingress which the laws and usages of the country had previously denied them. The same policy appears to have been fostered by the subsequent kings of the same dynasty until its consummation by Amasis (B.C. 569), when, in the language of Champollion Figéac, Egypt became at once Egyptian, Greek, and Asiatic; her national character was lost for ever; her armies were filled with foreign mercenaries; the throne was guarded by European soldiers, and continual wars completed the destruction of a tottering kingdom.†

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* Among the meagre facts which history has preserved in relation to those intrusive kings, the following is the most remarkable: "Sabakon (the first king of the Ethiopian dynasty,) having taken Boccoris (the legitimate sovereign) captive, burnt him alive." Manetho *apud* Cory, Frag., p. 126. Could any circumstances have rendered the Ethiopians more detestable in the eyes of the Egyptians than this first act of barbarian policy?

† Egypte Ancienne, p. 207.

CONCLUSIONS.

1. The valley of the Nile, both in Egypt and in Nubia, was originally peopled by a branch of the Caucasian race.

2. These primeval people, since called Egyptians, were the Mizraimites of Scripture, the posterity of Ham, and directly affiliated with the Libyan family of nations.

3. In their physical character, the Egyptians were intermediate between the Indo-European and Semitic races.

4. The Austral-Egyptian or Meröite communities were an Indo-Arabian stock engrafted on the primitive Libyan inhabitants.

5. Besides these exotic sources of population, the Egyptian race was at different periods modified by the influx of the Caucasian nations of Asia and Europe,—Pelasgi, or Hellenes, Scythians, and Phœnicians.

6. Kings of Egypt appear to have been incidentally derived from each of the above nations.

7. The Copts, in part at least, are a mixture of the Caucasian and the Negro, in extremely variable proportions.

8. Negroes were numerous in Egypt, but their social position in ancient times was the same that it now is, that of servants and slaves.

9. The national characteristics of all these families of man are distinctly figured on the monuments; and all of them, excepting the Scythians and Phœnicians, have been identified in the catacombs.

10. The present Fellahs are the lineal and least mixed descendants of the ancient Egyptians; and the latter are collaterally represented by the Tuaricks, Kabyles, Siwahs, and other remains of the Libyan family of nations.

11. The modern Nubians, with a few exceptions, are not the descendants of the monumental Ethiopians, but a variously mixed race of Arabs and Negroes.

12. Whatever may have been the size of the *cartilaginous* portion of the ear, the osseous structure conforms in every instance to the usual relative position.

13. The teeth differ in nothing from those of other Caucasian nations.

14. The hair of the Egyptians resembled, in texture, that of the fairest Europeans of the present day.

15. The physical or organic characters which distinguish the several races of men, are as old as the oldest records of our species.—*Transactions of the American Philosophical Society*, vol. ix., New series, Part I., p. 155.

NOTE.—I have taken frequent occasion to quote the opinions of the late Professor Blumenbach, of Göttingen, whose name is inseparably connected with the science of ethnography; but I have to regret that, up to the present time, I have not been able to procure, either in this country or from Europe, the last two memoirs which embrace his views on Egyptian subjects, and especially the work entitled, "Specimen historiæ naturalis antique artis operibus illustratæ." His views, however, as previously given to the world, have been repeatedly adverted to in these pages; and his matured and latest observations, as quoted by Dr Wiseman, appear to have confirmed his original sentiments. "In 1808," says Dr Wiseman, "he more clearly expressed his opinion, that the monuments prove the existence of *three distinct forms*, or physiognomies, among the ancient inhabitants of Egypt. Three years later he entered more fully into this inquiry, and gave the monuments, which he thought bore him out in this hypothesis. The first of these *forms* he considers to approach to the Negro model, the second to the Hindoo, the third to the Berber, or ordinary Egyptian head.—(Beträge zur Naturgerschichte, 2 ter Th. 1811.) But I think an unprejudiced observer will not easily follow him so far. The first head has nothing in common with the *Black race*, but is only a coarse representation of the Egyptian type; the second is only its mythological or ideal purification."—*Lectures on the Connexion between Science and Revealed Religion*, second edit., p. 100.

I thus place side by side the opinions of these learned men. With respect to Professor Blumenbach, I may add, that when he wrote on Egyptian ethnography there were no *fac simile* copies of the monuments, such as have since been given to the world by the French and Tuscan commissions; and again, that learned author had not access to a sufficient number of embalmed heads to enable him to compare these with the monumental effigies. With these lights he would at once have detected *an all-pervading physiognomy which is peculiarly and essentially Egyptian;* and in respect to which all the other forms,—Pelasgic, Semitic, Hindoo, and Negro, are incidental and subordinate; sometimes, it is true, represented with the attributes of royalty, but for the most part depicted as foreigners, enemies, and bondsmen.

With Egyptian *statuary* I am little acquainted. The only four years of my life which were spent in Europe were devoted almost exclusively to professional pursuits; and the many remains of Egyptian art which are preserved in the British and Continental museums, have left but a vague impression on my memory. How invaluable to Ethnography are the two statues of the First Osortasen, now in the royal cabinet of Berlin! Those I have not seen, nor the memoir in which Dr Lepsius has described them.

I have, for the most part, omitted any remarks on the intellectual and moral character of the Egyptians, because they would have extended my work beyond the limits prescribed by the present mode of publication. I have also avoided, as much as possible, those philological disquisitions which have of late years combined so much interest and discrepancy; but which are all important to Egyptian ethnography, and are daily becoming better understood, and, therefore, of more practical value. For an instructive view of this question, and many collateral facts and opinions, the reader is referred to the third volume of Dr Prichard's *Researches into the Physical History of Mankind*—a work which commands our unqualified admiration, both in respect to the multitude and the accuracy of the facts it contains, and the genius and learning with which they are woven together.

I look with great interest to the researches of Dr Lepsius at Meroë, as well as to those of my friend Dr Charles Pickering, who is now in Egypt for the sole purpose of studying the monuments in connexion with the people of that country; and, finally, it gives me great pleasure to state, that the profound erudition of the Baron Alexander de Humboldt is at this moment engaged in a work which will embrace his views on Egyptian ethnography, and give to the world the matured opinions of a mind which has already illuminated every department of natural science.—*Transactions of the American Philosophical Society*, vol. ix., New Series, Part I., p. 158.

On some particular Phenomena presented by the Mica Slate Formation at Flinsberg, in the Riesengebirge. By M. Gustav Rose.

This bed of mica slate is found in the gneiss to the north-west of the Riesengebirge, and extends from Raspenau as far as Wittich, and follows a curved line by Liebwerda, Schwarzbach, Flinsberg, Giehern, Querbach, Kunzendorf, Blumendorf, Hindorf, Alt-Kemnitz, as far as Voigtsdorf. Near the middle portion at Flinsberg, at the surface, it is rather more than a quarter of a German mile in breadth; it then cuts under sharp angles the two elevated summits of the gneiss of the Riesengebirge, which extend in a north-west direction from the two sides of the upper valley of gneiss, the two extremities of which consist of this same mica slate. In this part of the formation, the mica slate has in every respect the character of a mountain; a little further to the east it rises into the highest mountains, when its breadth diminishes, and it becomes narrower in proportion as we advance onwards.

M. Rose follows the rock throughout its whole extent and shews, by noticing all the localities where it is exposed, that the two sides of the gneiss valley do not correspond in their geognostic relations—that the southern limits of the mica slate are moved on the right side much more to the north than on the left side, although the strata on both sides proceed in absolute correspondence with the direction of the course of the river in the valley. These strata have, therefore, been torn asunder, and displaced at the time of the formation of the gneiss valley, and the eastern portion has been moved, but without changing the *allure* of the strata, along with the gneiss of Geierstein, more to the north than that on the left side. The same appearances, moreover, present themselves in all the valleys transverse to that of gneiss, but not on so large a scale.

Analysing all the facts, M. Rose remarks, that the strata of mica slate not only appear to have been cut and interrupted in their continuity by the valleys, but that it must have happened that the portions thus separated have been moved in certain directions; and if we now admit that the valleys among high mountains are nothing else than fissures, and that the strata must have been thus moved out of their position, it is certain that so conclusive a demonstration of this has not hitherto been offered.*

The author likewise describes the particular appearances presented by the mica slate in the vicinity of granite, appearances which he ascribes to the irruption of the latter, when the slate had already reached its crystalline state.

In conclusion, M. Rose shews that the mica slate of Flinsberg presents three phenomena deserving of attention; the direction in which it lies in relation to the summits of the system of mountains, the manner in which it has been moved at the time of the formation of the valley, and, finally, the changes which the mineralogical character of the rock has undergone at the points where it touches the granite.*

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* Our author has not alluded to the possibility of the portions of mica slate and gneiss being merely great distinct concretions, that is, *abgesonderte stücke*, and therefore *not moved.*—Ed.

Notice of Guano, from the Yorkshire Coast, and from the North Coast of Scotland. By JOHN DAVY, M.D., F.R.S. Lond. and Edin. Communicated by the Author.

The term guano seems likely to become a generic one, to designate all manures composed of the excrements of birds: it is thus already used in many parts of England and Scotland.

Although, from the nature of our climate, it cannot be expected that the home-guano can be equal in efficacy to the Peruvian and African, yet, considering that a considerable portion of the excrement of birds is very slightly, or not at all, soluble in water—and further, that it is a question whether the fixed and insoluble phosphates do not perform the most important part in promoting the growth of those plants into the composition of which they enter—there is sufficient reason that the home kind should not only not be neglected, but that attention should be specially directed to it.

With this persuasion, I purpose briefly to give an account of two portions of guano which I have lately received; for one of which I am indebted to Mr Hodgson, of Ayton Lodge, near Scarborough; and for the other to my friend Professor Jameson: the former collected on the Yorkshire coast; the latter brought from the Skerries in the Pentland Firth, procured by Robert Stevenson, Esq., civil-engineer, and Manager of the Lighthouses of Scotland and the Isles.

The Yorkshire guano, Mr Hodgson informs me, is the excrement of wild pigeons, which, in large numbers, frequent and breed in the limestone cliffs of Scarborough Head. About forty tons of it are collected annually, by men who follow the difficult and dangerous occupation of gathering eggs, and who, for that purpose, let each other down, by means of a "gin" or windlass, from the margin of the cliff, varying there in height from 50 to 200 feet. It is purchased by the farmers in the neighbourhood, at the rate of 1s. per bushel, or about 2s. 6d.

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* From l'Institut, No. 540, p. 154.

per cwt.; and has been used, from time immemorial, as a manure for grain crops, in the proportion of about six cwt. per acre, and with such effect, that it is held in great estimation for its fertilizing power.

It is of a light brown colour—a mixture of fine powder, bits of straw and chaff, and a little sand and gravel. It has a peculiar smell, but not ammoniacal till moistened and mixed with lime, when it emits this odour distinctly. From a coarse analysis of it which I have made, it appears to consist of—

10	Saline matter, soluble in water, in which the muriatic, sulphuric, and nitric acids were detected, with lime, potash, ammonia, and magnesia.
24	Organic matter, chiefly vegetable, destructible by fire, not soluble in water.
60	Matter not destructible by fire, of which 21 were soluble in muriatic acid, consisting chiefly of phosphate of lime, with a little carbonate of lime and magnesia; and 39 were insoluble, composed principally of siliceous sand and gravel.
6	Hygrometric or adhering moisture.
100	

This composition of the Yorkshire guano accounts, in a satisfactory manner, for its fertilizing effect, especially when applied to grain crops.

It may appear singular, that, whilst mention is made of nitric acid and soluble salts, as present in this guano, no notice is taken of lithic acid, of which, in combination with ammonia, as is well known, the urinary portion of the excrement of birds chiefly consists. It was sought for, but in vain; or, at farthest, only an obscure trace of it could be detected. This is not difficult of explanation, remembering that the lithate of ammonia is soluble in water, and that the guano examined had been exposed to the action of rain. In another specimen, collected, at my desire, from parts of the cliff protected from the weather, and for which, also, I am indebted to Mr Hodgson, I found a considerable quantity of lithate of ammonia.

The nitric acid present—probably in combination with lime—it may be conjectured, was derived, with the soluble salts, from an overhanging surface of limestone rock, and was either scraped off in gathering the excrement, or was washed down by the dropping of water, and absorbed and retained by the guano, supposing a period of dry weather to have preceded its collection.

It is worthy of remark, that both the Peruvian and African guano, although abounding in nitrogenous compounds, are destitute of nitric acid. This circumstance is strongly corroborative of the theory of the formation of nitre, in which carbonate of lime is held to be essential to the production of the acid, by exerting a certain influence in uniting its gaseous elements.

The guano from the Pentland Firth was in firm lumps, of a dirty brown colour, some of them speckled with white. It had a peculiar smell, not unlike that of sea-weed, and unmixed with any ammoniacal odour, till after having been triturated moistened with lime, when it gave off a pretty strong smell of ammonia, overpowering the odour first perceived. Broken up, after soaking in water, when it offered no resistance, and carefully examined with the microscope, it was found to consist chiefly of minute fragments of sea-shells and of sea-weed, with which were intermixed a fine granular matter, and particles of siliceous sand—leading to the inference that it was derived from birds that feed mostly on sea-weed, and on the smaller mollusca common amongst sea-weed. According to the information with which I have been favoured by Professor Jameson, the birds inhabiting the Skerries are "cormorants, and a few gulls and marrots." From a rough analysis, it appears to consist of about—

4	Matter soluble in water, chiefly muriate of ammonia, nitrate and sulphate of lime, with a trace of common salt.
28	Matter destructible by fire, a mixture of vegetable and animal matter, nearly insoluble in water.
60	Matter not destructible by fire, consisting of 30.6 carbonate and phosphate of lime, with a trace of magnesia, and a little sulphate of lime, and of 29.4 siliceous sand.
8	Hygrometric water, or adhering moisture.
100	

Considering the proportion of carbonate and phosphate of lime which this guano contains, as well as the saline matter soluble in water, and the organic matter destructible by fire, and capable of yielding carbonic acid during its slow decomposition, it may be pronounced to be of some value as a manure, and deserving of being collected. And, recurring to a preceding remark, I would lay the more stress on the value of manure of this kind, deprived of the greater part of its salts, and especially of its ammoniacal salts, by the action of rain, the earthy phosphates remaining, which water is incapable of dissolving,—seeing that a notion, far from correct, is commonly entertained, that guano, after exposure to rain, is rendered useless. Thus, in a letter from Ichaboe, recently published in the *Leeds Mercury*, descriptive of that remarkable islet, the writer of it (Mr J. Lees), after expressing his apprehension that the great deposits of guano will soon be exhausted, and that no new ones will be discovered, as he supposes that they must be limited to rainless climates, adds—" That many thousands of tons of guano, after having been taken in [shipped], were cast away, when it was discovered that the rains had caused its fermentation, and destroyed its properties." This is an opinion not less erroneous than one lately announced at a great agricultural meeting, that the effect of guano, as a manure, must be fugitive—depending on its volatile ammoniacal ingredients,—overlooking the non-volatile ammoniacal salts which it contains in large proportions, as well as the insoluble phosphates.

Considering, as has been already observed, these phosphates as not the least important of the ingredients of guano, the excrements of birds, wherever they have been accumulated, whether abounding in nitrogenous compounds, as in dry climates, or in the insoluble phosphates, as in rainy climates, must be valuable to the agriculturist, and are likely to repay the enterprising merchant who may import them. In the arctic and antarctic regions of the ocean, and those bordering on them, as birds, feeding on fish, there abound, it is probable that great stores of guano of the latter kind are laid up in accessible situations, and which may furnish cargoes to our whalers, and partly remunerate them when unsuccessful in their fishing enterprise. And, nearer home, as in Iceland, the Feroe Islands, and St Kilda, it is likely much useful guano might be collected, were the inhabitants who depend chiefly for their support on the feathered race to collect the excrement at the same time that they take the birds or their eggs.

The same view may be even farther extended. As the excrements of birds, without exception, when first voided, are rich in ammoniacal compounds, and contain more or less of phosphate of lime, birds, generally, must be admitted to be fertilizers—the effect being in proportion to their numbers,—in the instance of the solitary bird not perceptible, but in that of gregarious birds, especially in their roosting-places, very manifest. I have examined the soil under rookeries, and have detected in it ammonia and phosphate of lime. And as, under old rookeries, there must be an accumulation of the insoluble salts derived from the excrements of these birds, it hardly allows of question, that it will be advantageous to collect the soil so impregnated, from time to time, at proper intervals, and to employ it as a manure, restoring in this form to the fields a great part of what was taken from them by these useful birds, in the shape of worms and grubs. It is a pleasing circumstance in the economy of nature, that the sheltering shrub or tree, and the sheltered bird, benefit each other; that the excrementitious matter of the one, which, to the incurious and uninformed, may appear offensive, and a pollution, is perfectly fitted to contribute to the growth of the plant, and its beauty. In harmony with this, is another fact, one which I have lately ascertained, viz., that where there is no rain, and, consequently, where there can be no vegetation, there the lithate of ammonia, constituting the greater proportion of the urine of birds, is converted, by the action of the sun's rays, into a non-volatile but soluble salt, the perdurable oxalate of ammonia—one of the principal ingredients of the great depôts of American and African guano—instances of the most concentrated manure, hoarded in absolutely desert wastes, forming a genuine sinking fund for the agriculture of a country such as ours, wasteful of its natural manures.

THE OAKS, AMBLESIDE, *August* 31. 1844.

We are informed by our friend David Stevenson, Esq., Civil Engineer, that the deposit of guano on the Little Pentland Skerries, as mentioned by Dr Davy, is about 30 yards in length, 20 yards in breadth, and 1 foot in thickness; and that the amount of this manure on that spot alone is, therefore, probably about 200 yards, on about the same number of tons. We would recommend the proprietors of the coasts and islands of the north of Scotland to direct their attention to this subject.—EDITOR.

Professor Buckland on Artesian Wells.[*]

Professor Buckland said, he would at once proceed to the subject on which he had been requested to address the members of the Artesian Well Committee of this place. In his address, last Wednesday, to the Council of the Royal Agricultural Society, he had spoken of the capabilities of permanent agricultural improvement in Hampshire, and other southern counties of England, especially in the districts between the sea coast, and a line drawn from Dorchester through Salisbury and Winchester to London, including Wareham Heath, Poole Heath, the New Forest, and Bagshot Heath. If the improvement of these wastes by the mineral manures that lie beneath their surface, were taken up in a scientific manner, and on a large scale, by great proprietors, or by a land-improvement company, small portable steam-engines, and portable tram roads, might be employed, to raise from shafts in any part of this district, and transfer to profitable distances, and spread upon the surface, the chalk, and clay, and marl, that lie at various depths under the area of all these sandy wastes. Thus, the silt of the Humber has, with very great profit, been lately transferred by tram roads to be spread on the surface of barren peat; and in Norfolk, vast tracts of sandy rabbit warrens have, during the last half century, been converted to productive corn-fields, by adding to their surface a top-dressing of marl, clay, chalk, or shelly sand and gravel, locally called *crag*. The cost of such top-dressings of mineral manure need rarely exceed L.10 an acre, and the consequent benefit is the conversion of dreary deserts into permanently valuable arable land. On Lincoln Heath, where, not 100 years ago, a land-lighthouse was erected to guide the benighted traveller across a barren sandy waste, the application of scientific agriculture and capital had converted thousands of acres of unprofitable heath into pleasant and productive corn-fields. The chalk-hills, also, that form the wolds of Lincolnshire, and the wolds of Yorkshire, had been made rich by processes which were now beginning to be introduced in Hampshire.

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[*] The above is an account of an interesting Address to the Mayor, and Members of the Artesian Well Committee of Southampton, on the 27th of July 1844, by the Rev. Wm. Buckland, D.D., Professor of Geology, Oxford, &c.

On Thursday last, the Prussian minister had called the attention of the assembled agriculturists of England to the example of good farming that is set them by the most illustrious of living warriors, the Duke of Wellington, who had turned his glorious sword into a not less glorious ploughshare; and near Strathfieldsaye may now be seen rich fields of barley and turnips on naturally heavy clay lands, which, two or three years ago, were reeking with moisture, and incapable of that rotation of green and grain crops, which all good farming requires. The Duke of Wellington was, year after year, improving his clay lands, first by thorough-draining, which is the indispensable precursor of all other improvements; and after drainage, spreading large quantities of chalk over the surface of the clay. Not less than 1000 waggon loads of chalk had during the last year, been brought from the neighbourhood of Basingstoke to that of Strathfieldsaye.

Similar improvements of poor sandy soils may be made by laying upon them a good top-dressing of clay and chalk, in addition to ordinary manures; and geology had ascertained the existence of several kinds of marl and clay, and also of chalk, at various depths beneath the poor sandy heaths which form so large a portion of South Hants, and Dorset, and Berks, and under the whole of Bagshot Heath. The place of these clay beds is often indicated by the oozing of water and growth of rushes near the base of the sloping sides of the shallow valleys or combes that traverse these sandy plains, and are occasionally covered with peat. Between Christchurch and Poole, many such oozing streamlets point out spots from which, by the aid of a small steam-engine and tram-road, clay may be brought up to reclaim the sandy wastes around each of these centres of supply of fertilizing mineral manure; and the efficacy of this process had been shewn on a small scale near Poole, in little inclosures, made by a few industrious peasants. In Hampshire, he rejoiced in the occasion of recording a much greater example of improvements of this order, now in progress, on the property of the accomplished Baronet, who so worthily represents this county in Parliament, and who, like the noble and gallant Lord-Lieutenant of the county, has placed himself at the head of those who are engaged in the patriotic work of amending the productive capabilities of the soil. Between Southampton and his hospitable mansion at Hursley Park, Sir William Heathcote has already converted to good arable land, large inclosed portions of the sandy soils at Anfield and Cranberry Heath, by enriching them with the permanent mineral manures of clay and chalk. Sir W. Heathcote has also adopted, on the farm he occupies at Hursley, the practice of stall-feeding oxen, which is essential to produce the great quantities of manure that are required for the fertilization of all soils that are naturally poor, and without which, the improved fertility of the chalk and sandy lands in Lincolnshire could not be sustained. He has rendered a further inestimable service to the agriculture of Hampshire, by the first establishment, in this county, of one of those agricultural steam-engines, which are so common on large farms in Scotland and the North of England. The employment of a steam-engine is one of several causes of the great profit of farming in Scotland, and wherever it has been introduced in England. That erected by Sir W. Heathcote performs the work of thrashing, winnowing, grinding, and bruising corn, of cutting chaff and turnips, cracking bones and beans, turning a saw-mill, &c.; and thus leaves a large number of labourers free to be employed in the more profitable and improving work of cleaning and cultivating more highly the ancient corn-fields, of draining wet lands, and transporting chalk, and clay, and marl, to enrich the surfaces of sandy commons. Sir W. Heathcote had also dug wells at Hursley, which have near connection with the well now in progress on Southampton Common; and when this great and costly public work shall be completed, the level of its water will probably be found to oscillate in unison with the variations in the level of the water in the wells of Hursley.

The scientific search for water, and the scientific conversion of barren soils to fertility, were examples of the practical application of geology to the useful purposes of life; and the sciences of agriculture and civil engineering must obviously be imperfect in some of their most fundamental points, without a knowledge of the composition of soils, and structure of the earth.*

In all kinds of operations under ground, the necessity to the engineer of a knowledge of geology, and of the hydrostatic conditions of subterraneous water, would appear from every fact he was about to notice in the well on Southampton Common, and also in the very recent Artesian well at the Southampton Railway Station, and in wells at Otterbourn and Hursley, on the south-west of Winchester. He would, however, first inquire—whence came that inexhaustible subterranean supply of water which Providence had laid up in store, wherever the earth was habitable. On this part of the history of water he should say less, because he had given a summary of what was known on the subject, in a chapter of his Bridgewater Treatise, illustrated by diagrams, explaining the origin of springs and Artesian wells.

The sun now shining so bright and beauteous, drew up vapour from the surface of the ocean, which was held in a state of invisible solution in the air, until, condensed by cold, it fell in fertilizing drops upon the earth. By this sublimely simple natural machinery, supplies of *fresh* water were obtained from the *sea*, for the salt was not taken up with the vapour, except in an almost imperceptible degree. The mean quantity of rain which fell annually in England was about 31 inches, and nearly 3000 tons of water were deposited annually upon every acre, in a manner which the best watering-pot could imperfectly imitate. These fructifying waters descended from the air upon the earth in a state most favourable for vegetation, charged with minute quantities of sea-salt, together with ammonia and carbonic acid, all affording elements of nutrition to the vegetable kingdom. The water thus supplied at intervals by rain from the clouds, was disposed of in four different ways. The flood-waters of stormy weather, and the sudden meltings of snow, were rapidly restored by rivers to the sea. Another portion of the rain-water that fell upon dry land, was evaporated from the surface of the soil, and so again taken into the atmosphere, to mix with the vapour exhaled from rivers, lakes, and seas. A third portion supplied the drink and fluid nutriment of all animal and vegetable nature; and a fourth was disposed of to maintain the perennial supplies of wells, and springs, and rivers. M. Arago states, that it has been ascertained by an apparatus placed across the river at Paris, that not one-third of the rain that falls on the district that is drained by the basin of the Seine, returns directly by that river to the sea,—the remaining two-thirds being applied to the other purposes just mentioned. This most distinguished astronomer had directed special attention to the investigation of the economy of water in the natural world, and had illustrated it by the phenomena of the great Artesian well at Grenelle, near Paris. He had not only foretold that water would be found in this well, at an enormous depth below the chalk, but that it would rise and overflow the surface; accordingly, it has risen in a large column 30 feet above the highest part of Paris. M. Arago predicted also, that the temperature of this water would become gradually higher, increasing about one degree at every 45 feet below the surface. It now rises from the depth of near 1800 feet, at the temperature of 91° (Fahrenheit), warm enough to be applied to the heating of green-houses and hospitals.

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* The best little and cheap book he could recommend to farmers, for shewing the agricultural character and capabilities of the different soils and subsoils of England, and particularly of the chalk, and beds of clay and sands, that lie above and immediately beneath it, was "Morton on Subsoils."

In ancient days the difficult scientific problem of the origin of subterraneous water had occupied the attention of Aristotle and Seneca, and their opinion was, that water was supplied to springs by the action of central heat, causing it to ascend towards the surface of the earth. This theory cannot be true in the case of that large part of the earth's surface which is formed of stratified beds of porous stone, permeable by water, and alternating with impermeable beds of clay, through which no water can ascend or descend. The condition of a water-logged porous stratum thus placed between two beds of clay, through which no water can pass, may be compared to that of water enclosed in a tick or waterproof case, to form what is called a water-bed. We may, in imagination, extend indefinitely the size of this bed, containing water instead of feathers; and if we added to this water sand, or pebbles, or angular stones, the intervals of all these would be occupied by that portion of the fluid which was not displaced by the solid bodies thus immersed in it. Such a tick or bed-case full of stones and water would represent the permanently drenched and water-logged condition of all permeable strata below the level of the lowest springs by which their water can find issue. A sheet of such water-logged stone, or of permanently wet sand, is called by the French geologists a "*Nappe d'Eau*;" it is not a sheet of pure water, but a bed or sheet of sand or stones, whose interstices are filled with water, subject to the laws of hydrostatic pressure. The lowest regions of the chalk and of other porous strata are usually filled with such sheets of water, supplied by rain descending through innumerable cracks and fissures; and it was one of the infinite wise provisions we find in the natural world that the same water, which if placed in casks or open tanks, becomes putrid, continues fresh so long as it remains in the cavities and interstices of the strata of the earth.

The greatest number of ordinary wells are dug in shallow beds of gravel resting on the hollow surface of a subjacent bed of clay. Wells sunk to a greater depth through stratified rocks often afford larger supplies, but rise rarely to the surface; and in cases where they do so, they are called Artesian wells, from the circumstance of such artificial overflowing wells being common in Artois, the ancient Roman province of *Artesium*. The deepest well we know of this kind is that just mentioned, at Grenelle, near Paris, about 1800 feet deep; from which the water rises thirty feet above the surface, and at the temperature of 91° Fahrenheit. Less deep, but similar wells abound near London; and the Board of Woods and Forests was now erecting two large fountains in Trafalgar Square, to be supplied by two contiguous wells, in which it was expected that water would rise within 60 or 70 feet of the surface, in sufficient quantity to supply these fountains that have been prepared in the assurance of finding water. Other wells had been sunk in various parts of London, some into sheets of water pervading beds of sand and gravel that alternate with plastic clay, others into the still lower beds of chalk. In all these cases the water was forced up, by hydrostatic pressure, to various distances from the surface. At Brentford there were many wells that continually overflowed their orifice, which is a few feet only above the Thames;—in the London wells the water rises to a less level than in those at Brentford.

As the largest part of the earth's surface is composed of stratified rocks, the most frequent cause of water being supplied to wells, and springs, and rivers, was the alternation of beds of clay with porous and permeable beds of stone or sand. These alternating strata, having been originally formed in nearly horizontal positions, have been more or less displaced, and set on edge by volcanic forces, which raised them from the bottom of the sea. The greater part of these strata being porous and permeable by water, whilst beds of clay are impervious to that fluid, the residuary portions of rain-water (which are not disposed of by floods, or by evaporation, or by entering the bodies of animals or vegetables) are absorbed into the fissures and small interstices of the permeable strata, where they form subterraneous sheets or reservoirs of water for the sustentation of springs and rivers. About two-thirds of the habitable portions of the earth consist of stratified rocks, and the other third part of unstratified and crystalline rocks, such as granite, porphyry, lava, and other rocks of igneous origin. These also contain water in the countless cracks and interstices of their lower regions, and are intersected by innumerable fissures, which collect and transmit rain-water, and give origin to springs.

As persons who have no experience in such subjects may be surprised at the knowledge geologists profess to have acquired respecting the internal structure of the earth, he would endeavour to confirm the above theoretical explanation of the origin and supply of springs, by appealing to practical proofs, in the proceedings of water companies and well-diggers, and in the pounds, shillings, and pence, in the ledgers of manufacturers. In November 1840, notice was given of an application to be made to Parliament to obtain a new supply of water for London, from wells and water-works to be made at Watford, in the chalk. A company had been proposed to effect this object, which would, probably, have been carried, had not Mr Clutterbuck demonstrated, by a long-continued series of measurements of the water in the chalk hills of Hertfordshire, near Watford, that every drop of water taken from that neighbourhood would have been abstracted from the summer and autumn supplies of the river Colne, and would have robbed the proprietors of more than thirty mills upon this river and its tributaries, and the owners of the adjacent water-meadows, f rights which they had inherited from time immemorial. One intelligent manufacturer of paper, Mr Dickenson, who now supplies the paper for stamped letter-covers, and whose mills were on one of the tributaries of the Colne, had, during many years, found arithmetical evidence that the quantity of summer water in that river varied with the quantity of rain in the preceding winter. He could always tell in the end of February or March how much water there would be in these rivers in the following eight or nine months, and he regulated the contracts he made in every spring for paper to be delivered in the summer and autumn by the quantity of water in his winter rain-gauge. This rain-gauge, the invention of Dalton, being buried three feet below the surface, shewed that, except in December, January, and February, rain-water rarely descends more than three feet below the soil, so as to add anything to the supply that sinks into the earth to issue during summer, and form springs and rivers; and whenever Mr Dickenson found, by this instrument, that but little rain had fallen in the three months of winter, he proportionally limited his contracts for the following summer and autumn; thus proving the *practical* advantage of inductions from philosophy, and shewing that paper-making was dependent on meteorology, on hydrostatics, and on geology. In Germany, Mr Bruckman of Heilbronn, published, in 1835, an octavo volume on the Artesian wells in the valley of the Neckar, from which it appeared that there were manufactories in Wurtemberg, near Canstadt, where the mills were kept in work during the severest cold of winter, by means of the warm water from Artesian wells, which overflowed into the mill-ponds, and prevented them from freezing. And at Heilbroon, also, there were persons who saved the expense of fuel by conducting Artesian warm water in pipes through their houses and green-houses. In France, M. Héricart de Thury, a distinguished engineer, and president of the Royal Agricultural Society of France, has published a most interesting history of the Artesian wells in that country, all in theoretical accordance with the wells in Wurtemberg and England. Let those who doubt go to Grenelle, and see the majestic column of warm water from that philosophically predicted fountain, rising thirty feet above the surface, at the exact temperature foretold by Arago, and learn the correctness and value of practical deductions from geology, applied to the useful purposes of life.

The learned professor then explained the principles of hydrostatic pressure that are involved in the theory of the rise of water in common springs, and in Artesian and other wells, which he exemplified by reference to maps and diagrams representing sections of the London basin. In this and other geological basins, the position of a water-logged porous bed between two beds of clay may be illustrated by a tea-saucer placed within another tea-saucer, and having the narrow space between them filled with sand and water; if a hole were drilled through the bottom of the upper saucer, and a quill or small pipe fixed vertically in the hole, water would rise in this pipe to the level at which it stands within the margin of the lower saucer, its rise being caused by the same hydrostatic pressure that raised the water in the well on Southampton Common from the vast subterranean sheets of this fluid which exist in the fissured chalk-beds of the Hampshire basin, as they do also in the chalk under the basin of London. The rain that falls on the uncovered chalk within the area of these basins descends, by countless crevices, into the lower regions of the chalk strata to a level, where they are permanently charged with water throughout all their interstices and fissures, as the water charges the interspace between the two saucers just mentioned; and wherever a hole is bored, or a well sunk, into these water-bearing beds, through the impermeable strata that lie over them, the water will rise to the level of the lowest natural outlet or spring that gives vent to the overflowings of the sheet of water thus penetrated. As the streamlet that flows over the lower lip of the margin of a common pond prevents the further rise of water in that pond, so the springs that issue from chalk, and from all other water-bearing strata, prevent the permanent rise of subterranean water within the crevices of these strata much above the level of their respective springs.

The surface-line of any subterranean sheet of water may be ascertained by measuring a series of wells at distant intervals along the dip of the stratum under examination; and this subterraneous water-surface is usually found to be at its greatest height at the end of the rainy months of winter, and lowest at the end of the rainy months of autumn. In the village of Hursley, the water, after very rainy seasons, overflows from the wells of nearly every cottage; in the end of autumn, their water is usually more than forty feet below the surface. Observations by Mr Fowlie, the intelligent steward of Sir W. Heathcote, had discovered a sympathy between these village wells and three which have been sunk at a higher level in the park and farm-yard; and a similar sympathy may, ere long, be found between the Hursley wells and the deep well upon Southampton Common. The water they now extracted from the latter well was probably supplied by rain that sunk into the chalk in distant parts of the country; springs of fresh water often rose even from fissures at the bottom of the sea, and one near Chittagong was 100 miles distant from any land. M. Arago, speaking of the water in the well at Grenelle, near Paris, says it may come 40, 80, or 180 miles under ground to supply that well. An Artesian well at Tours rose with a jet that sustained in the air a cannon ball; the same jet has brought up a great quantity of seeds; and the nearest place at which these seeds could have entered the stratum below the chalk to come that distance under ground was thirty or forty miles off. There were two *swallow holes* at Hursley, where, at certain seasons of flood, the water is swallowed or engulphed into the chalk, and may carry down seeds with it, and it was not impossible that such seeds might one day rise in the well at Southampton Common.

Districts composed of chalk were, beyond all others, exempt from inundations, and absorbed unusual quantities of rain-water. Those persons who had seen Stockbridge may have remarked that many of the bridges were so low that even the ducks lower their heads as they swim under them. The bridges are low also at Salisbury and Winchester, because the chalk in their neighbourhood absorbs great part of that rain water which causes floods upon less absorbent strata. A rare exception to this rule occurred three or four years ago, at the village of Shrewton, on Salisbury Plain, in a severe winter, when the surface of the chalk was sealed up with ice. Nearly all the houses in this village were washed away by a flood, produced by the melting of snow, at a time when the ground, being frozen, could not, as it usually does, admit the water to the absorbing crevices of the chalk.

He would now call attention to the large and important spring, called Pole's Hole, which issues permanently, in quantity sufficient to turn a mill, at Otterbourne, distant about seven miles hence, between Southampton and Winchester. In this spring we have the nearest large natural vent, or outlet, which regulates the level of the subterranean waters of the chalk in that part of Hants, The level of this vent may, therefore, affect that of the water which rises in the well on Southampton Common; for if this water comes from the same bed of chalk that supplies the spring, or vent, at Otterbourne, it can rise to no great height above the level of this vent when the water is lowest, nor above the level of the wells at Hursley when the water is highest.* The capacity for transmitting water differs in different beds of the great chalk formation; some beds are fragmentary and incoherent, and through these the water passes rapidly; other beds are so continuous and solid, that little or no water can percolate them. In the boring at Grenelle, they found no useful water in the chalk, nor until they had gone down a considerable depth in the sandy and argillaceous beds of the green-sand formation below it; the lower chalk beds on Southampton Common may be equally destitute of water, and a continuation of the present borings many hundred feet more through the lowest chalk, into the green-sand formation, may possibly produce a jet like those from the same green-sand at Paris and at Tours; but perchance it may fail to increase materially the quantity of water that is already found, and which, if the facts that are said to be now observed in pumping from the present supply be correct, is sufficient to yield more than 40,000 gallons a-day. In 1842 a well had been sunk at Brighton in chalk, which, though but 97 feet deep, gave, by pumping with steam, 700 gallons of water per minute, and 347,000 gallons in 24 hours. At 80 feet, they cut into a water-bearing bed of chalk, full of fissures, from which the water gushed out abundantly. In this fissured stratum they made four horizontal galleries or adits, all of them intersecting so many small fissures or crevices, loaded with water, that further progress was soon impeded. The water in this fissured stratum was descending from the chalk hills of the South-Downs into the sea, which it enters by numerous springs along the shore near Brighton. In two of Sir W. Heathcote's wells at Hursley, the lowest bed of chalk was dry, and the water was obtained by making horizontal adits in a weeping fissured bed, a few feet above the bottom of each well. Had the downward digging on Southampton Common been stopped when the well arrived at the first bed of chalk that gave signs of water, and had lateral galleries been driven into that bed, these might have possibly have yielded

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* The water in the well on Southampton Common is said to rise at the present time (August 1844), to within 41 feet 7 inches of the surface. Should this level be much above that of the vent at Otterbourne, some water must either enter the well from the tertiary strata above the chalk, or enter the bore hole from fissured beds of chalk lower than that which has its lowest vent at Otterbourne; and such water-bearing lower beds must be separated from that which has this vent at Otterbourne, by intermediate beds of solid and impermeable chalk. All these beds must also

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be exempt from any of those great transverse fractures called faults or slips, which whenever they occur below the level of the vent of subterraneous sheets of water, may form channels of communication between the water in upper and lower porous beds that have an impermeable stratum between them.

a sufficient supply without boring to the present depth; but in such case the water would not have risen to the surface, so as to form an overflowing Artesian well. The further continuation of the present deep borings may, by possibility, intersect a fault, or large fissure, abounding in water, but it is much more probable it would not do so; and as it is impossible to drive out horizontal galleries from a bore hole, it might have been prudent to have driven them from that part of the well where the chalk first yielded the smallest streamlets of water.

Mr Clutterbuck had ascertained that a sympathy exists between deep wells more than a mile distant from each other in London. Every long-continued pumping in the well at Reid's brewery, in Liquorpond Street, was felt in the well of the New River Water Company, in the Hampstead Road, more than one mile from the brewery; and as the number of deep wells is continually increasing, each of which lowers the level of those next adjacent to it, the general level to which water will now rise under London has been reduced many feet below that at which it stood in the first made well. Mr Clutterbuck had further observed that the surface line of subterranean sheets of water was not horizontal, like the surface of a lake, but inclined at a rate varying from 14 to 20 feet per mile, in consequence of friction caused by the particles of the strata through which those sheets of rain-water descended with retarded motion to be discharged by springs. This inclination of the subterranean water line in the chalk of Hertfordshire had been found by Mr Clutterbuck to be nearly at the rate of 20 feet per mile in the chalk between Sir John Sebright's park at Beechwood and the town of Watford, and 14 feet per mile in the chalk under tertiary strata in some parts of the basin of London. The engineers of the Southampton railway had found a similar fall of about 16 or 17 feet per mile in the wells at the railway stations between Basingstoke and Southampton.

He would now congratulate this town on the recently discovered evidence of another valuable source of water, of great importance to its inhabitants. A true Artesian well, overflowing from a depth of 220 feet, had just been completed at the railway station. The water overflows from this well, at the rate of ten gallons per minute, at five feet above the surface; at the depth of 100 feet it supplied to the pumps 48 gallons per minute; and it is probable, that wherever they might bore to the same depth under any house or street in the town, water would rise to nearly the same height as that to which it rises at the railway station. This water comes from a sandy stratum in the tertiary formations that overlie the chalk, which forms the foundation of the geological basin in which Southampton stands.

In conclusion, Dr Buckland alluded to the many admirable contrivances by which the Creator has adapted both the waters and the land to supply the wants of all organized beings He has placed upon this beautiful world. The whole of what is now dry land had been upraised by the agency of earthquakes and volcanic forces from the bottom of the sea; and the entire surface of the globe has been rent by millions of fractures and fissures destined, to serve an important purpose, as reservoirs and conduits, for pouring everlasting supplies of water into the springs and rivers that run among the hills. Amidst apparent confusion, science finds method and order; from seemingly discordant and perturbate elements, she extracts evidences of concord and harmony, and benevolent design, teaching lessons of gratitude to the Almighty Author of every natural good, the giver of every moral benefit and religious blessing.

On Fossil Fishes.

1. *Classification.* 2. *Illustrated by comparative Anatomy.* 3. *Importance in Geology.*

Agassiz having now finished his great work on Fossil Fishes, we have much pleasure in laying before our readers the following observations on this important work:—

1. *Classification.*

1. In a zoological point of view, the most important fact is, that M. Agassiz's work makes us acquainted with upwards of a thousand species of fishes, more than the half of which are described in detail, and carefully represented of the natural

size. Such a number of species added to the inventory of the animal kingdom is an important acquisition, a real advancement in zoology, particularly in a class so little known as that of fishes.

So many new species could not enter into the systems of Ichthyology, without causing a necessity for new changes, both by discovering types entirely new, and making us better acquainted with the affinities of various groups and families. M. Agassiz, accordingly, has not confined himself to the establishment of a number of species, genera, and even families. He has created a classification entirely new, founded, in a great measure, on the importance of fossil fishes. Cuvier admits two great divisions in the class of fishes, the osseous fishes, and the cartilaginous fishes. M. Agassiz likewise separates the osseous from the cartilaginous fishes, of which he composes his first order, that of Placoïd; but he further divides the osseous fishes into three other orders of the same value, so that the class of fishes is thus divided into four orders, which are, 1*st*, The Order of the *Placoïds*; 2*d*, The order of the *Ganoïds*; 3*d*, Order of the *Ctenoïds*; 4*th*, Order of the *Cycloïds*. This classification is not founded on the skeleton, like that of Cuvier, but on the nature of the exterior integuments, the scales. M. Agassiz lays it down as a principle, that the exterior integuments of fishes are the reflection of their internal organization. Proceeding on this, he examines the different families of the class of fishes in relation to their scales; and he finds in the conformation of the exterior cuirass a multitude of characters, on which he founds his classification. In this point of view, it is, first, to be observed, that all the osseous fishes, with the exception of certain genera, are covered with corneous scales, while the skin of the cartilaginous fishes is furnished with plates or spines of a particular form, known by the name of shagreen in Sharks, and *boucles* among Rays. The scales of the osseous fishes are constructed on an entirely different plan: the differences are even so decided, that they have appeared sufficient to M. Agassiz to serve as a basis to the three orders Cycloïd, Ctenoïd, and Ganoïd. The Cycloids and the Ctenoids, which comprehend nearly all the osseous fishes of our era, are both possessed of corneous scales;

but they differ in this, that the one, the Ctenoïds, have the posterior edge of the scales denticulated, while in the Cycloïds the same edge is entire. The author endeavours to prove that this distinction, apparently insignificant, is, notwithstanding, founded in nature; that it is the expression of a fundamental feature, which, in like manner, appears in the other parts of the body. It is thus that the fishes possessing denticulated or pectinated scales, are in general stuck over with spines on the head, the operculum, and various parts of the body, while the others, the Cycloïds, are smooth fishes, without any armature. M. Agassiz considers the family of perches and its allies as the type of his order of Ctenoïds; and the family of carps, salmon, and pikes, as the type of the order Cycloïd. This division, therefore, corresponds to a certain point with Cuvier's division, into Acanthopterygians and Malacopterygians; and this coincidence will be sufficient of itself to prove the necessity that exists for separating these two types, since we arrive at the same result by ways entirely opposite. Is M. Agassiz's method more successful in this than that of Cuvier? We scarcely believe that it is; and already the researches of other naturalists have pointed out fishes belonging to the same family, some of which have the scales of the cycloïds, and others the scales of the ctenoïds. Yet we willingly admit, that, for the study of fossil fishes, the distinction derived from the scales is of greater practical value than that which is founded on the structure of the dorsal fins.

M. Agassiz's second order, Ganoid, appears to us much better founded. Two fishes exist in the Nile and in the rivers of South America, which have at all times embarrassed ichthyologists. One of them, that of the Nile, is known under the name of Bichir (*Polypterus Bichir*); the other, that of America, under the name of the Osseous Pike (*Lepidosteus*), because in its exterior it resembles our pike. Both these fishes are covered with scales, of a form and structure quite peculiar. Instead of being placed one over another like tiles on a roof, as among ordinary fishes, they are simply placed beside each other, and their surface is covered with a coat of enamel, which forms a very solid cuirass. M. Agassiz has examined these fishes in an anatomical point of view, and differences appeared in their skeleton not less remarkable than in the scales and soft parts of the body. Notwithstanding that, he would have hesitated to insulate these fishes completely from the other great families; and particularly because, when their small number is considered, it would have been contrary to all methods, to place them on the same rank with the Placoïds on the one hand, and the osseous fishes on the other. But what the study of recent fishes did not warrant, was justified by the study of fossil fishes. We have met with an ichthyological fauna possessed neither of the characters of the osseous nor of the cartilaginous fishes, but which remind us in every respect of the Bichir and Lepidosteus. Thus it is that these two genera of fishes, which appear so exceptional in the existing creation, really form a type by themselves, which, however small in number in our days, is not the less, on that account, the expression of an entire order of things. By grouping round these fishes all the numerous fossils whose scales are of the same structure, M. Agassiz forms of them his division Ganoïd, which already contains many hundred species, and which promises always to become more numerous; for, as we shall afterwards see, it is it which predominates in all the epochs anterior to the chalk. M. Agassiz has determined many distinct families in this order; the two principal are that of the *Sauroïds*, to which the Lepidosteus and the Bichir belong, and that of the *Lepidoïds*, which comprehends inoffensive, and probably omnivorous fishes, similar in their physiognomy to our carps, but which have no representative in the present era.

To each of the four orders a volume is devoted, accompanied with a magnificent series of plates, in which all the species described are represented. In the descriptions, which are often very much in detail, the author has not confined himself to the indication of the particular characters of the ichthyolite with which he is specially occupied. It is seldom that he does not find an opportunity of introducing some reflections of general interest on the family to which the fish he is describing belongs, on its distribution or its mode of association with other fossils, and on the circumstances in which it is probable the animal lived. Besides, the study of the families or genera which have representatives in the present period, is commonly preceded by a description of the skeleton of a living species, in order to facilitate and complete the knowledge of the fossil species, which, not being usually preserved entire, are on that account more difficult to determine. If, on the contrary, he treats of families wholly extinct, the author has endeavoured to afford a similar advantage by restored figures, representing the fish with the form and ornaments which may be supposed to have belonged to it, judging from the preserved remains.

2. *Fossil Fishes important in an anatomical point of view.*

M. Agassiz's work is not a less important contribution to anatomy than it is to zoology and geology. Obliged to study minutely, not only the external form, but also all the parts of the skeleton in living fishes, in order to determine the detached analogous pieces met with in the strata of the earth, the author was under the necessity of tracing, with the greatest care, the numerous modifications which these same bones undergo in the different families of the class of fishes. There can be no doubt that this is the most difficult part of the work; for in no other class of the vertebrata is the osseous frame-work so variable. We have only once to examine the head of a fish in order to perceive the difficulty of referring all the pieces to a constant type; for not only are the bones of the head more numerous among fishes than among the other vertebrata, but they are combined in so many different manners, that it is very difficult to detect their true relations. On the other hand, age induces very considerable changes, not only in the form and dimensions of the different bones, but even in their structure, and to such a degree that the same bone often cannot be recognised in the different stages of life. Hence the necessity of studying the development of all the parts of the skeleton, in order to be in a condition to distinguish with certainty the essential characters from the secondary characters—what is constant from what is transitory. Considered in this light, the researches which the author has undertaken, in connection with M. Vogt, on the embryology of the Salmonidæ, must have afforded him great assistance; they have, above all, given him the means of appreciating the relative value of the different organs, and the rank which the families ought to occupy in the ichthyological scale.

These comparative studies have led our author to the discovery of a capital fact, which had not previously been announced, namely, that a remarkable parallelism exists between the development of the individual and the development of the entire class in the order of time. In the earlier periods of embryonic life, no vertebral column exists. This organ is represented, in embryos, by a gelatinous cord, which is called the dorsal cord. It is around this organ, which continues for a longer or shorter period in all fishes, that the vertebræ are formed in the shape of osseous rings. These rings insensibly enlarge, and always encroach more and more on the dorsal cord, which at last altogether disappears in the majority of fishes. There are certain types, however, the sturgeon, for example, where it remains during the whole life; accordingly, this fish has no vertebræ, and the apophyses rest immediately on the dorsal cord. Now, M. Agassiz makes us acquainted with the fact, that this is likewise the case with the fishes of the ancient epoch. All of them possess distinct spiny apophyses, often very strong and completely ossified, but they exhibit no traces of distinct vertebræ; whence the author concludes that they were deprived of these organs, and that the dorsal cord continued in them during the whole period of life, as in the sturgeon.

We may make a single remark with regard to the relative superiority of the living types. Here, also, embryology everywhere reveals to us a wonderful parallelism. There is no fish, however imperfect it may be, whose organization does not correspond to one or other of the phases of life in the most perfect types. Let us take the lamprey for an example, or that still more imperfect fish known by the name of *Amphioxus* or *Branchiostoma*, and which was arranged by Linnæus among the Vermes, so widely does it differ from ordinary fishes. Of these two types, the first has only the cartilaginous base of the cranium; the second is completely destitute of it, and the dorsal cord extends as far as the extremity of the muzzle. The first has only a single fin, more or less separated; in the second, this fin uniformly surrounds the entire body. Finally, neither the one nor the other is possessed of true jaws. Now, it will be observed that our most perfect fishes, such as the salmon, have a period in their lives when they are at this point of development; only in the one this period is temporary, a progress towards a state of higher development; while in the others it is the extreme term of development. These considerations are of great importance in a philosophical point of view, especially when we consider the application that may be made of them to the other classes of the animal kingdom. They have also served as a guide to our author in the rank he assigns to the different families of fishes according to their organization.

The direction thus given to his studies necessarily led M. Agassiz to discuss many questions of more general interest, respecting which anatomists are not yet agreed. What he says respecting the formation of the cranium appears to us particularly interesting; and no one, we think, can read the following reflections without feeling their force. He says,—"I have shared with a multitude of other naturalists the opinion which regards the cranium as composed of vertebræ. I am, consequently, in some degree called upon to point out the motives which have induced me to reject it. This I shall do the more freely, since we may now discuss the question in all its aspects without fear of wounding the feelings of others.

"M. Oken was the first to assign this signification to the bones of the cranium. The new doctrine he expounded was received in Germany with great enthusiasm by the school of the philosophers ef nature. The author conceived the cranium to consist of three vertebræ, and the basal occipital, the sphenoid, and the ethmoid, were regarded as the central parts of these cranial vertebræ. On these alleged bodies of vertebræ, the arches enveloping the central parts of the nervous system were raised, while on the opposite side were attached the inferior pieces which went to form the vegetative arch destined to embrace the intestinal canal and the large vessels. It would be too tedious to enumerate in this place the changes which each author introduced in order to modify this matter, so as to make it suit his own views. Some went the length of affirming that the vertebræ of the head were as complete as those of the trunk; and, by means of various dismemberments, separations, and combinations, all the forms of the cranium were referred to the vertebræ, by admitting that the number of pieces was invariably fixed in every head, and that all the vertebrata, whatever might be their organization in other respects, had in their heads the same number of points of ossification. At a later period, what was erroneous in this manner of regarding the subject was detected; but the idea of the vertebral composition of the head was still retained. It was admitted as a general law, that the cranium was composed of three primitive vertebræ, as the embryo is of three blastodermic leaflets; but that these vertebræ, like the leaflets, existed only ideally, and that their presence, although easily demonstrated in certain cases, could only be slightly traced and with the greatest difficulty in other instances. The notion thus laid down of the virtual existence of cranial vertebræ did not encounter very great opposition; it could not be denied that there was a certain general resemblance between the osseous case of the brain and the rachidian canal; the occipital, in particular, had all the characteristic features of a vertebra. But whenever an attempt was made to push the analogy further, and to determine rigorously the anterior vertebræ of the cranium, the observer found himself arrested by insurmountable obstacles, and he was obliged always to revert to the virtual existence.

" In order to explain my idea clearly, let me have recourse to an example. It is certain that organized bodies are sometimes endowed with virtual qualities, which, at a certain period of the being's life, elude dissection, and all our means of investigation. It is thus, that, at the moment of their origin, the eggs of all animals have such a resemblance to each other, that it would be impossible to distinguish, even by the aid of the most powerful microscope, the ovarial egg of a craw-fish for example, from that of true fish. And yet who would deny that beings in every respect different from each other exist in these eggs? It is precisely because the difference manifests itself at a later period, in proportion as the embryo develops itself, that we were authorized to conclude, that, even from the earliest period, the eggs were different; that each had virtual qualities proper to itself, although they could not be discovered by our senses. If, on the contrary, any one should find two eggs perfectly alike, and should observe two beings perfectly identical issue from them, he would greatly err if he ascribed to these eggs different virtual qualities. It is, therefore, necessary, in order to be in a condition to suppose that virtual properties peculiar to it are concealed in an animal, that these properties should manifest themselves once in some phase or other of its development. Now, applying this principle to the theory of cranial vertebræ, we should say that if these vertebræ virtually exist in the adult, they must needs shew themselves in reality, at a certain period of development. If, on the contrary, they are found neither in the embryo nor the adult, I am of opinion that we are entitled likewise to dispute their virtual existence.

"Here, however, an objection may be made to me, drawn from the physiological value of the vertebræ, the function of which, as is well known, is, on the one hand, to furnish a solid support to the muscular contractions which determine the movements of the trunk, and, on the other, to protect the centres of the nervous system, by forming a more or less solid case completely around them. The bodies of the vertebræ are particularly destined to the first of these offices, the neurapophyses to the second. What can be more natural than to admit, from the consideration of this, that, in the head, the bodies of the vertebræ diminish in proportion as the moving function becomes lost, while the neurapophyses are considerably developed for protecting the brain, the volume of which is very considerable, when compared with that of the spinal marrow? Have we not an example of this fact in the vertebræ of the tail, where the neurapophyses become completely obliterated, and a simple cylindrical body alone remains? Now, may it not be the case, that, in the head, the bodies of the vertebræ have disappeared; and that, in consequence, there is a prolongation of the cord only as far as the moving functions of the vertebræ extend? There is some truth in this argument, and it would be difficult to refute it *a priori*. But it loses all its force the moment that we enter upon a detailed examination of the bones of the head. Thus, what would we call, according to this hypothesis, the principal sphenoid, the great wings of the sphenoid, and the ethmoid, which form the floor of the cerebral cavity? It may be said they are apophyses. But the apophyses protect the nervous centres only on the side and above. It may be said that they are the bodies of the vertebræ. But they are formed without the concurrence of the dorsal cord; they cannot, therefore, be the bodies of the vertebræ. It must, therefore, be allowed that these bones at least do not enter into the vertebral type; that they are in some measure peculiar. And if this be the case with them, why may not the other protective plates be equally independent of the vertebral type; the more so because the relation of the frontals and parietals vary so much that it would be almost impossible to assign to them a constant place."

Microscopic studies had also to furnish their contingent to M. Agassiz's work, since the researches of Mr Owen on the structure of the hard parts of animal bodies, and especially the teeth, have demonstrated that a perfect regularity and a wonderful uniformity exist in the arrangement of the smallest fibres of these organs. The knowledge of these details is particularly valuable for the study of the fossil Placoïds, of which we possess only teeth and fin rays, the other parts of the skeleton not being fitted for preservation in a fossil state on account of their soft nature. Even in the existing fauna, there is a group of Sharks, whose teeth are so like each other in external form, that it is almost impossible to distinguish them; for example, the teeth of the Lamnæ and those of the true Sharks (*Carcharias*), or the teeth of the true Sharks and the Carcharodons. But examine their internal structure, and you will find remarkable differences. The same thing applies to the rays of the fins, in so much that hereafter it will be sufficient to cut a slice from a tooth or a ray, and to examine it with the microscope, to ascertain correctly to what animal it has belonged. We may, in like manner, determine by means of this ingenious proceeding, even the smallest fragments, provided they are capable of being cut into fine slices. We congratulate M. Agassiz on having devoted a certain number of plates to the study of these details, which appears to us destined continually to acquire more importance in palæontology. The same affinities, the same transitions which take place from one genus to another, and from one family to another, likewise reappear in these details. It is thus that the Sauroïds, which of all fishes approach nearest to Reptiles, have teeth of a structure very similar to that of the Ichthyosauri, while the Sharks, which occupy a lower degree in the scale, shew quite a different structure.

3. *Importance of Fossil Fishes in a geological point of view.*

It is by their geological importance that M. Agassiz's ***Researches on Fossil Fishes*** are particularly destined to create a sensation. Even in the earlier parts of the work, the author, by comparing the fishes of the different formations, found an opportunity of throwing new light on the relative age of many of these formations. He was thus led (to mention only a single example), by the study of the fishes enclosed in the slates of Glaris, to demonstrate that this deposit, previously regarded as belonging to the most ancient sedimentary formation, the greywacke, is much more recent, and is a part of the cretaceous formation. Another more general result of M. Agassiz's work is, that not only all the fossil species are different from those that live in our days, but that they are equally distinct as we proceed from one formation to another. Besides this, the author does not only limit these differences to the great formations; he establishes them also in the diverse stages of the same formation. It is thus that he does not find identical species in the lias and the upper Jura, in the inferior and superior deposits of the chalk, in the ancient and recent portion of the Tertiary class, &c. Now, the natural consequence of these differences is, that the entire creation has been renewed at these different epochs by a direct intervention of the Creator. Such a conclusion will perhaps seem rash; but it appears that observation tends to confirm it more and more; at least M. D'Orbigny arrives at nearly the same results by the study of the testaceous animals.

Along with these differences, so constant and so regular, the author likewise discovers a genetic connection between the type of fishes and that of the other classes of the vertebrata, when their development is considered throughout the different geological epochs. The considerations which he attaches to this fact are as new as they are bold; for they tend to nothing less than to prove, that fishes are in some sort the primitive trunk from which, in the course of time, the different other classes of the vertebrata have been detached. It is, indeed, curious to observe, that fishes have been, during the whole Transition period, the only representatives of the vertebrata. There is, in particular, a type of voracious fishes, which arrives at its *apogée* in this period; namely, that of the Sauroids, which seems to have then shared with the sharks the empire of the seas, in so much that this period may be justly called the *reign of fishes.*

Only at a later date, during the Triassic period, reptiles appeared, and they soon became, in their turn, the lords of the creation, principally in the Jurassic formation, when the Ichthyosauri and the Plesiosauri inhabited the scarcely formed coasts of Europe, It was then the *reign of reptiles.* A multitude of fishes belonging to new species existed along with these reptiles, but they lost the pre-eminence; and if many of them attract observation by their large size, they are still far from equalling the power of the great Sauroids of the carboniferous epoch.

With regard to mammifera and birds, M. Agassiz makes their reign commence only with the tertiary epoch; and here, perhaps, his system is open to criticism, for he is not ignorant that there exist mammifera of the Jura period (the fossil Didelphis of Stonesfield); and we believe that he has distinctly admitted that it is to the type of the mammifera that these singular remains ought to be referred. If he takes no account of them in his system, it is, no doubt, because he regards them as an exception; and, in fact, it is curious that we meet with no other remains of mammifera in the subsequent strata of the Jura formation and of the chalk formation, while they appear suddenly in extraordinary abundance, and of colossal dimensions, in the tertiary epoch. With regard to birds, M. Agassiz has himself informed us that unquestionable traces of them exist in the slates of Glaris. Now, while we acknowledge the ingenuity of regarding the succession of types in this manner, it would still be of much importance, in the interest of the system, that neither mammifera nor birds were found in the secondary epoch; for if it is the task of a system to assign a reason for all the phenomena which it embraces, it is evident that precursors of the kind alluded to, such as the Didelphis of Stonesfield and the birds of Glaris, offer difficulties of no easy solution.

At the top of the scale of the vertebrata, our author places man as the crown of the creation, and whom he regards as the object and end of the creation. According to M. Agassiz, it is with reference to man that this successive and continuous development, from fishes to reptiles, from reptiles to birds and the mammifera, and from the latter to man himself, has been effected. But this process of perfection has not been effected by filiation—by direct procreation, since all the species are different as we go from one formation to another. The bond which unites them is not a material bond; it exists in the mind of the Creator, who had in view an intelligent being whom he designed to be sovereign over all. M. Agassiz thus expresses his thoughts on this subject:—"The progressive connection, as if by the links of a chain, of the four classes of vertebrate animals, is a fact which contrasts, in every respect, and in a very striking manner, with the uniform and parallel development of all the classes of the invertebrata. The gradation of the vertebrates is so much the more remarkable, on account of its direct connection with the advent of man, whom we may consider not only as the term, but also as the object of all this development. Let us first regard fishes, which appeared first. Plunged in a medium denser and less mobile than the atmosphere, they are always found in conditions of existence less varied than those of terrestrial animals. Their body is all of a piece; their head is not detached from the trunk, of which it is nothing else than a simple prolongation; their organs are obtuse, and their faculties very limited; their members placed in pairs are not the principal organs of motion, and there exist only very slight relations between individuals of the same species. Reptiles, which succeed fishes in the order of time, present us with a more perfect organization; their head is more or less detached from the rest of the body, and can even be raised above the horizontal line which the trunk still forms; the members in pairs, when they exist at all, are true locomotive organs; they cannot, however, elevate the whole mass of the body, which is dragged, rather than carried, by the feet. These animals are evidently superior to fishes in the development of the organs of the senses and intellectual faculties; more varied relations between individuals of the same species are accordingly found among them. In birds, which come next in order, we observe a very remarkable development. Without attempting to demonstrate the indisputable superiority of their organization over that of the two preceding classes, I shall insist only on this single fact, that their bodies can be completely raised from the ground by means of their locomotive members, which present, in their disengagement from the body, the most striking contrast with the locomotive appendages of fishes and reptiles. We constantly find in birds two kinds of locomotive members, wings for flight, and feet for walking or swimming; and, what is curious, when they rest, these animals support themselves only on their posterior limbs, the body and head inclined forward and upwards. Among the mammifera, we find, for the first time, an organization in which all the limbs harmonize—all of them maintaining the body in an elevated position. We need not be surprised, however, to find, in this class, types as varied as the Cetacea, Quadrupeds properly so called, the Cheiroptera, and the quadrumana; for, after a development as eccentric as that of birds, what can be more natural than to find the mammifera reproduce, in their sphere, forms which recall inferior types, as if it were definitively to overcome the relations which connect animals with the soil, before attaining to the noble gait and free movements which characterise man, and which permit him to elevate his face towards his Creator—to contemplate the entire universe—to perceive the laws which regulate it, —and to prostrate himself with gratitude and love before Him to whom he is indebted for such marvellous prerogatives."

The class of fishes, considered in itself, has likewise undergone numerous modifications during the series of geological ages, from the period of the transition formations down to our own times. Here, as in all the other classes of the animal kingdom, the fossil species bear a greater resemblance to the living species, in proportion as they belong to strata more recent; and each new formation is a further approach towards the actually existing state of things. The most important change in the entire class of fishes, has taken place at the end of Jura epoch. Up to that period, all the fishes had a peculiar physiognomy, in general very different from that which we now perceive them to possess; no other kinds were to be met with but the Ganoïd and the Placoïd. It was not till the time of the chalk formation that the two other orders, the Ctenoïd and the Cycloïd, which almost exclusively prevail in the present creation, made their appearance. The first types of these orders belong, for the most part, to extinct genera, allied to our Clupeæ and Tunnies. In this epoch, fresh water fish were still wanting. The fishes of the tertiary epoch are much more nearly related to those of our own times; a great number belong to genera now existing: we find true Tunnies, true Clupeæ, true Anchovies, true Smelts, and fresh-water fishes well characterised, such as Pikes, Leucisci, Tenches, Loaches, Gudgeons, &c., but neither Trouts nor Salmon. On the other hand, the Ganoïds become more and more rare in the tertiary formations. In a word, the Ichthyological Fauna of the tertiary deposits, whether viewed as a whole or in its details, presents the greatest analogy with that of our own times. In order to shew more conspicuously the signification of these different changes, the author has represented them, in a very ingenious manner, in a pictorial sketch, which indicates the appearance of the different families, and their development, relatively to the different eras.—(*Plate 3d, contained in this No.*)

It is, in general, to the tertiary fossils that those geologists who do not admit marked differences between the Faunas of the different epochs, have recourse, in order to establish, according to their views, the filiation of species across the different formations. They have even founded on the proportional number of living species of mollusca, which they pretend to have discovered in the strata of this period, a division of the tertiary class into Eocene, Miocene, and Pliocene formations. Now, if these identities had been real, they ought to have appeared equally among fishes. This was the capital point to establish. The fishes of the celebrated locality of Monte-Bolca had, it is true, been referred by Volta, without exception, to species actually living in the Mediterranean ; but it was easy to see that the determinations of the author of the *Ittiolitologia Veronese* were not the result of sufficient study; many naturalists had pointed out his errors even by a simple comparison of the plates. In order to be perfectly certain in this respect, it was of importance to compare the originals themselves. This M. Agassiz has done, with the greatest detail, in the Museum of Paris, where the collection of Count Gazzola, and the greater part of the originals of Volta's work, are preserved. He was not long in discovering that all the species were new, and that about the half belonged even to extinct genera.

M. Agassiz has arrived at nearly the same results with regard to the fossil fishes of another deposit, equally celebrated, the species of which had also been regarded as identical with those of our own times; I mean the fishes of Oeningen. The formation of Oeningen is a fresh-water deposit of more recent date than Monte-Bolca. The fishes it contains are very similar to those which now live in the Lake of Constance, and almost all belong to the same genera. Now, when we consider how little our Leucisci or *poissons blancs* differ from each other, we might fear that the analytical method employed by Agassiz would not be sufficient. Fortunately, the fishes of this locality are in general admirably preserved, so well that we can study the details of their skeleton with as much precision as that of a living species. From the minute comparison our author has made of these fossils with the fishes of the lake of Constance and the basin of the Rhine in general, it appears that not only are these fossils different from their living analogues, but also that they equally differ from the fossil species of the other great hydrographical basins, and in particular, from the species of Ménat in the basin of the Rhone. Now, in order that it could happen thus, it is necessary to admit that at the period of the deposition of these formations, the two basins of the Rhone and the Rhine were already separated ; for if they had communicated with each other, and if the fishes which now inhabit them were the direct descendants of the fossils of Oeningen and Ménat, it would follow that we ought no longer to meet with species peculiar to them either in the basin of the Rhone or in that of the Rhine. Now, every thing leads us to believe that the lake of Constance, as well as the greater part of the Swiss lakes, were produced by dislocations posterior to the deposition of the tertiary formations; and that being the case, how could the fishes of Oeningen survive catastrophes which have produced such modifications in the form of the surface of Switzerland? The consequence of these facts is obvious. If we succeed in demonstrating that certain basins, like certain terrestrial regions, are inhabited by peculiar species not found elsewhere in contemporaneous deposits, we must thence conclude that the creation has been not only renewed at different geological epochs, but also that the successive creations have been more or less local; that is to say, the species have been created in the places which they inhabit, and that a limit has been assigned to each which it does not overpass, as long as it remains in its natural conditions. It is only man, and a small number of species he has associated with him, that are not subject to this general law. And as the migrations of these same species have taken place under the direct influence of man, we may thence conclude that they did not take place in the anterior ages.*

On the Cause of the Electricity of Steam. By G. A. Rowell, Esq. Communicated by the Author.†

The cause of rain, evaporation, and atmospheric electricity, having engaged my attention for many years, I endeavoured, in two papers read before the Ashmolean Society, 1839‡ and 1841,§ to shew that evaporation is caused by the increase of the surface of particles of water by expansion, and that thus having a greater capacity for electricity, they are buoyed up by their coating of electricity as a bullet may be buoyed up in water by a coating of cork, and that *no evaporation at low temperatures could go on without electricity*; that the vapour so raised into the air, when condensed, becomes surcharged with electricity, and thus remains suspended until the surcharge escapes, either as lightning, or else imperceptibly, to the earth, when the remaining coating of electricity being insufficient to buoy up the particles of vapour, they fall as rain, &c.; and that it is possible to cause rain at will by raising electrical conductors to the clouds, by means of balloons, and thus enabling the surcharge of electricity in the clouds to escape to the earth.

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* From Bibliothèque Universelle de Génève, No. 100, p. 334-356.

† Read before the Ashmolean Society, February 26. 1844.

‡ Vide Report of the British Association, Glasgow Meeting.

§ London and Edinburgh Philosophical Magazine, vol. xx., p. 45.

The discovery of the electricity of steam I considered a strong support of these opinions; but a theory having been proposed by Dr Faraday, who explained the electricity of steam as caused by the friction of particles of water carried along by the steam rubbing against the solid matter of the passage through which the steam is escaping from the boiler, —the following is an attempt to shew that the electricity of steam is not caused by friction, but by its expansion, on escaping from the boiler, thus carrying off electricity, and rendering the boiler (if insulated) negative, the steam again giving off its positive electricity when condensed; and that the phenomena of Dr Faraday's experiments will support this hypothesis.

One experiment, which I believe tells against the theory of friction is as follows:—"An insulated wire was held in the stream of steam issuing from a glass or metal tube, about half an inch from the mouth of the tube, and was found to be unexcited; on moving it in one direction, *a little further off, it was rendered positive*, on moving it in the other direction, *nearer to the tube, it was negative*." In addition to this, both Mr Armstrong and Mr Pattison, in their experiments, found the greatest development of electricity at some distance from the boiler, in some cases five or six feet.

I cannot conceive how this phenomenon can take place if the excitement is caused by friction of the particles of water in the tube, as in that case I believe the strongest development of electricity would be at the mouth of the tube or boiler; but it fully agrees with the hypothesis that the phenomenon is caused by the expansion and contraction of the particles of steam.

All the experiments on the subject shew, that the steam within the boiler is not electrified, and that the electrical development takes place on its escape from the mouth of the tube. At this point, there is an enormous expansion of the steam; and it then takes up its portion of electricity, according to its expanded surface, in the same proportion as the electrical state of the boiler, or rather the issue tube. If the boiler or tube be insulated, they will be rendered negative; the steam at this point is so also; but, as it begins immediately to condense, it is, at a short distance, neutral; and, on a further condensation, and consequent diminution of surface, the steam becomes positively electrified.

The cause of the increase, through friction, of the electricity of steam, is probably from its bringing a greater quantity of the steam in contact with the issue tube; thus enabling a greater portion of the steam to take up its coating of electricity than could be the case if escaping from a round smooth aperture; as, in that case, owing to the non-conducting powers of *high pressure* steam, only the exterior particles of the column of steam could take their full coating of electricity.

The presence of water in the tube may increase the electrical development, by rendering the connecting and issue tube a better conductor of electricity from the boiler to the mouth of the issue tube.

The necessity for the issue tube being a good conductor of electricity is shewn by the experiments of Dr Faraday, who says, "A metal, glass, or box-wood tube, well soaked in distilled water, being used for the steam issue, the boiler was rendered well negative, and the steam highly positive; but if a quill or an ivory tube be used, the boiler received *scarcely any change*, and the stream of steam is also in a neutral state."

This must be owing to the difference in the conducting power of the various tubes, and not to the difference in the friction they occasion, as metal, wood when well soaked in water, and glass, from its becoming damp from the steam, are good conductors, and would supply the escaping steam with electricity; but quill and ivory being non-conductors, and having a tendency to resist dampness, would prevent the supply of sufficient electricity to cause any strong development.

Every insulated substance held in the current of steam from ivory or quill tubes became negatively charged, from the steam taking off a portion of their electricity.

That electricity cannot be obtained from currents of low pressure steam, may be accounted for by the increased conducting power of steam in this state preventing any development of electricity in the condensed steam, by conducting the electricity back to the boiler the instant any accumulation takes place : even the addition to *high pressure steam* of any saline or other substances (which increases the conducting power of water) prevented electrical development.

It is difficult to account for the absence of electricity when the valve of the boiler was lifted, in Dr Faraday's experiments, as both Mr Armstrong and Mr Pattison performed most of their early experiments from the safety valves of several boilers, and Mr Armstrong states that on one occasion "the engine was rendered *intensely* negative by a copious emission of steam from the valve." It may be owing to the small pressure on the boiler used by Dr. Faraday.

With respect to the cause why oil of turpentine, olive oil, &c., renders the steam negative, I can form no opinion, but believe that any substance which would reduce the conducting power from the boiler to the mouth of the tube, in any great degree, would render the stream of steam negative, by preventing the particles of steam obtaining their coating of electricity.

The increase of electricity, with the increase of pressure on the boiler, may be accounted for; as the expansion of steam on escaping from the boiler increases also with the pressure.

Miscellaneous Observations on Animal Heat. By JOHN DAVY, M.D., F.R.S. L. and E.*

1. *On the Temperature of the Pelamides.* 2. *On the Temperature of Man in advanced age.* 3. *On the Effect of Air of different Temperatures on Animal Heat.* 4. *On the Effect of Exercise on the Temperature of the Body.*

I. *On the Temperature of the Pelamides* (*Pelamys Sarda*, Cuv. and Val.)

Fishes generally are commonly considered as cold-blooded. In a work published in 1839, I have stated particulars tending to shew that this commonly received opinion is not universally correct, and that fishes of the genus Thynnus, with some others of the Scomber family, may be inferred to be an exception†.

As this inference was founded chiefly on reports of fishermen, it appeared very desirable to determine by actual thermometrical measurement what is the exact temperature of fishes of this family.

Hitherto, although watching for opportunities, and promised the aid of friends favourably situated, I have not been able to make any observations of the kind required, excepting on one species of these fishes, the Pelamides, the *Pelamys Sarda* of Cuvier and Valenciennes. The Pelamides, like most of its congeners, is migratory in its habits. In the early part of summer it appears in the sea of Marmora and the Bosphorus, and in August in the Black Sea, from whence, after spawning, it returns in September and October, on its passage to the Mediterranean. It is caught in the same manner as the Tunny.

In June 1841, whilst at Constantinople, I visited a fishing station for this fish, in an inlet of the sea of Marmora, and was present when a small capture was made, enabling me to ascertain the temperature of four specimens. This was done the instant they were taken out of the water, being in a boat alongside the net, by introducing a thermometer with a projecting bulb, through a small incision, into the muscle of the back, about an inch and a half, and immediately after into the cavity of the abdomen. In three instances, the thermometer in the back rose to 75° Fahr.; in one to 74°; in all, in the abdomen, it rose to 73°. The Pelamides were of moderate size, between two and three feet long. The air at the time was 71°; the sea at the surface 68°: but probably at the depth from which the fishes were taken, it was a few degrees lower, the descending current of the Bosphorus then being, where coldest, at 62°.

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* Phil. Transactions, Part I. for 1844. Received November 2.—Read December 11, 1843.

† Researches, Physiological and Anatomical, vol. i. p. 218.

Supposing that the water from which they were taken was 62°,—and it might have been lower, as the Pelamides swim in deep water,—the temperature of this fish would appear to be about 12° above the medium in which it swims, and at least 7° above that of the surface.

This result seems in accordance with the inference, that all fishes are not cold-blooded. In the work already referred to, reasoning from the smaller size of the respiratory nerves of of the *Pelamys Sarda,* compared with those of the Tunny, I offered the conjecture that its temperature would be found less than that of the Tunny, and somewhat higher than that of fishes of other orders with still smaller respiratory nerves, a conjecture which the observations described may be adduced as confirming.

In connection with their temperature, my attention was directed to the blood of these fishes. I have been able to examine it only in three instances, and that partially, viz. the Sword-fish, the *Pelamys Sarda,* and the common Tunny. Considering the great difficulty there is in obtaining the subjects for experiment under favourable circumstances for examination, imperfect as were my results, I am induced to offer them now.

The Sword-fish appears to abound less in blood than the Pelamides, and the Pelamides less than the common Tunny; and, accordingly, the muscles of the former two are of a much lighter colour than those of the latter.

The blood of the Tunny is very rich in red particles: this is indicated not only by its appearance, but also by its specific gravity, which I have found as high as 1.070. The blood tried was taken from a fish, caught in the sea of Marmora, that weighed between two and three hundred pounds.

The blood of the Pelamides appears to be less rich in red particles than that of the Tunny, but more than that of the Sword-fish: I have not ascertained its specific gravity. The specific gravity of the blood of the Sword-fish I have found to be 1.051; the fish from which the blood was taken was caught in the Bosphorus, in the month of December, and was of large size.

Under the microscope the appearance of the red particles of the blood of these three fishes is very similar. They are commonly thin oval discs (very soft), containing oval nuclei: a few circular discs are intermixed with them. The medium dimensions of those of the Pelamides were about $\frac{1}{2000}$th of an inch by $\frac{1}{3000}$th; of the Sword-fish, about $\frac{1}{5000}$th by $\frac{1}{4000}$th; and of the Tunny, about $\frac{5}{8000}$th by $\frac{3}{8000}$th.

That the red particles constitute that portion of the blood which is chiefly concerned in the production of animal heat, is now generally admitted. What a contrast appears, in comparing the blood of the fishes under consideration, with that of some of the colder, especially of the cartilaginous kind, in which it is very small in quantity, accompanied by a proportionally diminutive heart, and poor in red particles! The blood of the *Squalus Acanthias* I have found to exceed in density only a little its serum, one being of the specific gravity 1.030, the other of the specific gravity 1.027.

Whether the peculiar constitution of the red particles operates in any way in promoting their union with oxygen, seems to be deserving of consideration. It may be thrown out as a conjecture, that the circumstance of their possessing nuclei may have an effect of the kind, supposing, which is possible, the blood corpuscle and nucleus, or containing and contained part, to be in the electrical relation to each other of positive and negative. If it be objected to this, that, as regards nuclei as well as size, there is an analogy between the blood-corpuscle of fishes, birds, and reptiles, the temperature of which commonly is so very different, it may be answered, that in all these classes such a constitution of blood-corpuscle may be designed for the same end; and that birds partly owe their high temperature to it; and that in reptiles and fishes, in most of which the proportion of red particles is small, were the constitution of blood-corpuscle different, it would be inadequate to perform the part required of it.

2. *On the Temperature of Man in advanced age.*

Not aware of any observations having been published on the temperature of man in advanced old age, I have been induced to institute some trials, the results of which I shall now briefly describe.

1. 91 years of age; feeble on his legs, but in pretty good health; a native of Grasmere, in Westmorland, where he has always resided, in easy circumstances, cultivating his own land. In June, when the temperature of the air was 60°, a thermometer placed under the tongue rose to 99°·5; his hands were warm; his pulse at the wrist 48, strong, intermitting. The observation was made at 2 P.M.; he had dined at noon. On the 28th of the October following, his temperature was again tried, about the same time of day, when the open air was 42°, the air of his room 52°; now, under the tongue, the thermometer was 98°.5; the pulse 56°; his state of health much the same as before.

2. 88 years of age, also a native of Grasmere, where he has mostly resided, as a day labourer; is pretty firm on his feet, but troubled with chronic cough and difficulty of breathing. In June, when the temperature of the air was 60°, a thermometer placed under the tongue rose to 99°.5; his pulse was 56, and rather feeble; he had dined three hours previously. On the 28th of October, an hour after dinner, when his pulse was 70, the thermometer under the tongue was 98°; the air of the room 55°. In February, about three hours after dinner, when his pulse was 44 and feeble, the temperature under the tongue was 96°. This was on the 27th; the air then of his room was 44°; the open air about 32°, after a heavy fall of snow, and a sharp frost of several days' duration. The old man was feebler than in the summer and

autumn; and though he did not complain of cold, his hand felt cold.

3. The wife of the preceding, the mother of several children, 76 years of age; hale for her years, but blind from cataract, complicated with amaurosis. Her temperature, tried at the same time as her husband's, in June, was found under the tongue to be 98°.5, her pulse 78, and pretty strong. Tried again in October, it was found to be 98°, with a pulse of 70; and again in February, on the 27th, it was found to be 99°, her pulse being 80.

4. 87 years of age; a native of Ambleside, where she has commonly resided; feeble, but, excepting chronic cough, in tolerable health. On the 26th of October, at 3 P.M., the temperature under the tongue was found to be 98°.5; her pulse 84, and pretty strong; the air of the room then was 57°, the open air about 42°.

5. On the same day, and in the same village, tried the temperature of another old inhabitant, 92 years of age. The thermometer under her tongue stood about 98°; it could not be determined with perfect exactness, on account of the tremulous motion of her head, which also affected the limbs, preventing the counting of her pulse; her general health was pretty good.

6. An inhabitant of Ambleside, by trade a hatter, 89 years of age, hale, able to walk to church. On the 27th of October, when the air of his room was 56°, the outer air 42°, his pulse 64, strong and regular, the thermometer under his tongue stood at 98°. Observed again on the 27th of February, at 1 P.M., just after dinner, when the outer air was 32°, the air of his room 54°, the temperature under his tongue was found to be 99°.5; his pulse 70.

7. The temperature of his wife, two years younger, taken on the 27th of October, was 98°.5; her pulse was 88, irregular; she was very infirm, and suffering from asthma.

8. A native of Scotland, 95 years of age, now residing in Ambleside, where he has been many years, always in good health, still tolerably strong and active. On the 28th of October, found the temperature under his tongue 98°.5; his pulse 56, intermitting; the air of his room 57°. The old

people in all the preceding instances, at the time the observations were made, were sitting by their fireside, as is their usage in the cool climate of Westmorland, the greater part of the year, and all of them, with one exception, seemed to be comfortably warm; the poorest of them were not in want.

Old age is commonly represented as cold, and the temperature of the body is commonly supposed to diminish with advancing age. The results of the preceding observations generally are not in accordance with this opinion; they seem, on the contrary, to shew, that the temperature of old people, at least as regards the deep-seated parts, of which the tongue at its base may be considered as some indication, is rather above than below the average temperature of middle age, taking that to be about 98° of Fahr. Nor, perhaps, is this surprising, when we reflect, that most of the food consumed by old persons—and their appetite generally is good—is probably chiefly employed in administering to the function of respiration, being very partially expended in meeting the waste of the body.

Probably in very advanced old age, as in very early infancy, the power of resistance to cold is feeble, and the temperature of the body is easily reduced on exposure. An observation which I made, many years ago, in Ceylon, would seem to be confirmatory of this. At seven o'clock in the morning, when the air was 72°, I tried the temperature of an old man, almost a century old, and of a boy about twelve years old, both cool, being thinly clad, and out of doors; the temperature of the old man under the tongue was 95°; in the axilla, 93°; that of the boy under the former, 98°; and in the latter 96°.5. The observation, too, on the old man in Grasmere, made in February, in cold weather, is also favourable to this conclusion; whilst those made at the same time, on the other two old persons in stronger health, seem to shew, that, provided there is a vigorous action of the heart, and free circulation of the blood, the temperature of the body is easily maintained.

3. *On the Effect of Air of different Temperatures on Animal Heat.*

As from observations made on man on entering the tropics, and within the tropics on descending from a cool mountainous district to a hot low country, it would appear that his temperature, as measured by a thermometer placed under the tongue, is liable to fluctuate,—rising one or two degrees in a warm atmosphere, and falling as much on entering a cool one,*—it seemed probable that like differences of effect might be produced by air kept at different degrees of temperature in buildings in this country.

In the autumn of last year, when going through the cotton manufactory of Deanstone, in the neighbourhood of Doune, in Stirlingshire,—an establishment admirably conducted, and in the highest order,—I availed myself of the opportunity to try the temperature of a few individuals in relation to this question. In the room called the "piecing-room," where a high temperature is always required on account of the kind of work,—a temperature kept up by means of warm air and steam,—when at 92°, I found the thermometer placed under the tongue of one man who had been at work there about six hours, rise to 100°.5; and of another, who had been there the same time, to 100°: the former was 52 years of age, healthy, his pulse 64; the other 33 years of age, in pretty good health, but liable to acidity of stomach; his pulse 78.

In an adjoining room, where the temperature of the air was 73°, the thermometer placed under the tongue of a young woman rose to 99°; and in a large room, where 300 persons were employed in weaving, and where the temperature of the air was 60°, the thermometer placed under the tongue of another healthy young woman rose only to 97°.5.

Few as are these observations, they seem to warrant the conclusion that a high temperature of even a few hours in the heated air of a room is capable of raising the temperature of the body above its usual standard, in accordance with what had been anticipated from the effect of different degrees of atmospheric temperature.

In further confirmation of the same, I may briefly state the results of multiplied observations made on the temperature of the same individual. The subject of them was of middle age, in good health, under whose tongue the thermometer commonly was stationary at 98°, when neither suffering from heat nor cold. The place where they were made was Constantinople,—the climate of which capital, it may be observed, is exceedingly variable,—often cold in winter and the early spring, and commonly very hot in summer,—liable to great vicissitudes from its situation on the confines of two seas, in regard to warmth very different in character during a great part of the year. The observations were begun early in March, and were continued at intervals till the latter end of July. During this time the thermometer in the open air ranged from 31° to 94°, and the temperature observed under the tongue from 97° to 99°.5. It may not be amiss to mention some particular instances.

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* *Op. Cit.* i. 169.

On the 5th of March, after having been exposed several hours in an open boat on the Bosphorus, with a strong wind at 43°, the thermometer placed under the tongue stood at 97°.

On the the 11th of the same month, when the ground was covered deeply with snow, and the thermometer in the open air at 7 A.M. was 31°, and in a bed-room 45°, the temperature under the tongue was found to be 97°.5.

On the 3d of April, when the thermometer in the room, with the window open, was 66°, under the tongue it was 98°.5.

On the 17th of July, when the thermometer was 87°, under the tongue it rose to 99°.5. On the 21st of the same month, when the air was 87°, the temperature under the tongue was 99°.5; and on the 28th, when the former was 94°, the latter was 99°.

During the hot weather of July, it may be deserving of remark, that the pulse was less affected than the respiration, which, habitually about sixteen in the minute, was now commonly fourteen, and one day did not exceed twelve.

It may also be mentioned that attention was paid to the temperature of the extremities, and also to that of the urine, and that commonly it was found of highest temperature when the tongue and extremities were of lowest temperature: thus, on the 5th of March, when the thermometer under the tongue was 97°, the feet and hands cold, in the urine it rose to 101°; and on the 28th of July, when under the former it was 99°.5, in the latter it was the same.

Do not these observations, besides tending to confirm the preceding conclusion, for which they were brought forward, viz. that the temperature of the body rises and falls in a perceptible manner with the temperature of the air, lead also to the further conclusion, that the tendency of a high temperature of atmosphere is to raise the temperature of the surface and of the parts adjoining the surface, in a somewhat higher ratio than the deep-seated organs; and of a low temperature of atmosphere to raise the temperature of the deep-seated parts, whilst that of the surface is subjected to undue reduction from the cooling agencies to which it is exposed, directed, as it were, in both instances, for a beneficial result, on the principle of compensation?

4. *On the Effect of Exercise on the Temperature of the Body.*

This subject of inquiry, notwithstanding its manifest importance, has been much neglected; indeed, I do not know of any work in which any precise information is to be obtained respecting it.

The observations which I have to offer are fewer than I could wish, and more limited; they were made at Constantinople in 1841, at intervals between February and August, and had for their object mainly to endeavour to determine the effect of moderate exercise in walking, on the temperature of the body. The individual on whom they were made was the same as was mentioned in the last section. The particular observations are the following:

February 19th, at 1½ P.M., air of room, 60°; before walking, feet cold, temperature between the toes 66°; under the tongue 98°; urine 100°. At 5½ P.M., open air 40°; just returned from a walk, gently warmed by the exercise; feet and hands warm; the former 96°.5, the latter 97°; under the tongue 98°; urine 101°.

March 2d, at 4½ P.M., open air 50°; air of room 66°; feet and hands moderately warm; the former 75°, the latter 81°; under the tongue 98°; urine 100°. At 5½ P.M., after having walked pretty quickly an hour, a gentle perspiration produced, the hands and feet hot, found the latter 99°, the former 98°; under the tongue 98°; the urine 101°.5.

March 20th, at 5½ P.M., open air 42°; returned warm, after a walk of three hours: the hands, which had worn warm gloves, were 99°; feet 97°; under the tongue 98°; the urine 101°.5.

April 7th, after a walk of three hours in the open air, between 60° and 70°, returned at 5 P.M., gently perspiring: the hands were 94°; the feet 96°.5; under the tongue 98°.5; the urine 100°.5.

May 27th, at 6½ P.M., after a walk of an hour and half, the air 68°, returned slightly perspiring; the hands were 95°; the feet hot; under the tongue 99°.5; the urine 101°.5.

May 28th, air 65°; under the tongue before taking exercise 98°.5: after a walk of four hours and a half, gently perspiring, under the tongue 98°; hands 93°; feet 97°.5; urine 100°.5

September 13th, at 4 P.M., the open air on the shore of the Bosphorus 76°; ascended in about twenty minutes, without stopping, the steep side of the hill, called the Giant's Mountain; on reaching its summit, when profusely perspiring, the pulse was 102°, usually about 52°; the hands 98°; under the tongue 98°. The pulse of another individual in company, of about the same age, also profusely perspiring, was 138°; thermometer under his tongue 98°; and in the hand the same. After descent, the pulse of the former was 94°; thermometer under his tongue and in the hand 98°.5; the pulse of the latter was 112°; the thermometer under his tongue 98°.5; both only gently perspiring.

What is the inference from these observations? Do they not seem to indicate that whilst moderate exercise promotes the diffusion of temperature and its exaltation in the extremities, it augments very little, if at all, the heat of the deep-seated parts? And considering the blood as the heating medium, warmed itself chiefly by respiration, is not this what might be expected, reasoning on the subject? By active exercise, the pulse and the respiration are both accelerated; more oxygen, it may be presumed, is consumed, more heat is generated; the blood is made to circulate more rapidly, and is sent in larger quantity into the extremities, and where, in consequence, the excess of heat is conveyed and expended, and its accumulation in the central and deep-seated organs prevented, affording another striking example of harmonious adaptation.

The same thermometer was employed in making all the observations described in the paper; and in every instance, in stating the results, allowance has been made for error in its graduation, carefully determined by comparison with a standard instrument, one belonging to Professor Forbes of Edinburgh, and for the use of which I have been indebted to his kindness.

THE OAKS, AMBLESIDE,
Nov. 1. 1843.

Researches on the Situation of Zones without Rain, and of Deserts. By M. J. FOURNET, Professor in the Faculty of Sciences of Lyons.

In the present state of science, meteorologists distinguish various zones, more or less exactly parallel to the equator, and subject to as many distinct laws, with reference to the mode of distribution of rain according to the seasons. We proceed to make these known, premising, once for all, that in this preliminary statement, as well as in the whole of the memoir, we shall invariably retain for the seasons of both hemispheres the name which they receive in ours. Thus, for example, the summer will be regarded as composed of the months of June, July, and August, and the winter of the months of December, January, and February, whatever part of the globe may be under consideration. This definition was necessary, both because the epochs of dryness of the southern hemisphere are the humid epochs of the northern hemisphere, or conversely; and because the expressions intended to define the seasons become complicated by other local denominations, which, not being very precise, contribute to various ambiguities that are but too frequent in the narratives of travellers.

The first of the zones in question, that of *two annual rains*, having a great tendency to pass into *continual rains*, is placed nearly at the equator, with the exception of some deviations, which it is unnecessary to specify at present. There then come the bands of *hemi-annual rains*, comprised between the equinoctial line and the tropics. In these it rains during the six months following the period of the sun's passing the zenith; that is to say, there are *summer rains* in the northern hemisphere, and *winter rains* in the southern hemisphere. These *semestrial rains* degenerate into *trimestrial rains* near the tropics. This intertropical arrangement is perfectly regulated, so that it may be said, in a general manner, that to absolute dryness, lasting some months, succeed almost daily rains during the other season. In this manner are constituted the *seasons of suns and of clouds* of the Indian of the Orinocco; and it is only the presence of high chains of mountains, or the vicinity of coasts, which produces disturbances in the phenomena of this description.

Without the tropics, Von Buch was the first to point out the existence of zones termed *sub-tropical*, in which the rains become the reverse of what they were in the bands already mentioned; that is to say, they are *hyemal* in our hemisphere, and *estival* in the southern hemisphere; in other words, they occur in each of these situations when the sun is in the opposite hemisphere, and as examples of this, we may cite Algeria and the Cape of Good Hope. The arrangement here no longer possesses the extreme regularity which characterises that in the tropics; for sudden falls of rain and storms interrupt from time to time the uniformity of the dryness of other periods of the year, just as the rains are subject to frequent interruptions.

Further to the north, in our hemisphere, matters become still more complicated by the intercalation of irregular and more frequent rains; but taking the sums of several years, we find that the months characterised by the largest quantities of pluvial water are those of *spring and of autumn;* such is the arrangement which prevails in the whole of the south of Europe, as far as about the latitude of Paris.

Lastly, beyond this, and towards the North Pole, these rains of *spring and of autumn* unite so as to produce a maximum in the *summer season*, the reverse naturally taking place in the other hemisphere.

The preceding results can be rendered more palpable by the assistance of curves, in order to draw which, it is only necessary to raise on an equatorial line equidistant perpendiculars representing the different months. Those which represent the months of spring, of summer, and of autumn, fill the intermediate space, in such a manner that the summer occupies the middle. This first sketch will be completed by the different latitudes; and by co-ordinating the pluviometrical indications by means of these axes, we shall have for our hemisphere, first of all, on the equatorial line, the rectilineal band of nearly perpetual rains; there will then come a curve, whose two branches, starting from points at the equator, represented by the months of spring and of autumn, will converge towards the tropic in the months of summer, and all the interior of this arch being shaded, will express the semestrial rains degenerating into trimestrial rains towards the summit of the curve. Lastly, concentrically with the preceding, there will be a second curve, whose branches having their origin to the north of the tropic, at points represented by the winter months in the latitudes of Algeria, will afterwards pass by points represented by the months of spring and of autumn in the latitudes of the south of Europe, and will become, in the parallels which are rainy during the summer months, a perpendicular to the equator, prolonged from the latitude of Paris to the North Pole.

This mode of expressing succinctly the results of observation, of course does not take into consideration local disturbances, upon which I do not mean to insist at present, as they form the subject of a very extensive separate investigation, with which I have been occupied for several years. It possesses, however, the advantage of exhibiting a very remarkable phenomenon—that, namely, of the abrupt transition which presents itself from the zones of the trimestrial rains of the tropics, to that of the trimestrial rains of the opposite seasons of the sub-tropical zone of Von Buch. Now, a sudden leap of this kind not being in harmony with the ordinary laws of continuity of nature, I have investigated the causes of the anomaly, and have found that observation makes it disappear, and shews us that the two preceding zones, far from being in immediate contact, are, on the contrary, distinctly disjoined by bands of absolute dryness, at least in the portion of the globe constituting the region of the trade-winds, with which we shall, in the first place, occupy ourselves; we shall, afterwards, examine the phenomena connected with the regions subject to the monsoons.

Africa presents a very manifest demonstration of this circumstance. In the northern part of that continent, the band of absolute dryness is represented by the Sahara, which we may regard as prolonged, without interruption, from the Atlantic to the Red Sea, and having a breadth which the tropic divides into two nearly equal parts. In this manner, by uniting under a collective name the Sahara properly so called, the two deserts of Nubia and those of Egypt, the denticulations of the band would reach to 15° of north latitude, encompassing the ramifications of the central chains of the continent; while, on the opposite side, being bounded, for a part of its course, by the masses of the Atlas, it would, nevertheless, send off branches to the Mediterranean, as far as 13° of north latitude, by the lower parts of Egypt, and the salt plain of the Syrtis. Von Humboldt assigns to its surface, not including Darfour and Dongolah, an extent of 194,000 square leagues—that is, more than double the extent of the Mediterranean, which is 77,300 square leagues; but it must not be supposed that all this immense breadth belongs, properly speaking, to the *Sahara-bela-mâ* of the Arabs, nor that it is in a state of absolute dryness, although the hottest climates are situated under the tropic of Cancer, and as far as four or five degrees to the north. The true physiognomy of the desert only presents itself progressively, by the diminution of the vegetation, which, in proportion as the rains become more rare, degenerates from the condition of forests to that of brushwood, and is afterwards replaced by steppes, and, finally, by the sands, pebbles, or naked rocks of the central portions nearest the tropic.

It is thus that, in the western portion, at Timbuctoo (lat. 17° 50′ north), the rains, so abundant near the equatorial zone, are already rare, and the soil is poor; at El Araouân (lat. 20° north), the rains are still less frequent, and hardly any traces of verdure are visible; soon afterwards, the desert is reached, with its boundless horizon, its fiery sky, its rocks, its sands, and the moveable parts of its surface in which the winds hollow out valleys, raising, at the same time, pillars of sand, whose fall is the only representation of showers. At the northern limit of this immense land of desolation, there appears, at first here and there, spaces covered with plants, near El-Arib (lat. 28° north); these become more numerous under the influence of some hyemal rains which are still but little abundant in the steppes of Beled-el-Djerid (country of dates), where, nevertheless, the *wadis* (valleys with temporary torrents), and the vicinity of the Atlas, put an end to the aridity. Here there is no longer a desert properly so called, —for who would venture to give this name to a country where one single tribe, that of the Arbâa, is 40,000 strong! In the time of the splendour of the caliphs, this Beled-el-Djerid was of great importance, on account of its castles, its fortresses, its towns, its gardens, its forests of palms, its commerce, and its activity. At the present day, Ain-Modhy has been able to resist the power and perseverance of Abd-el-Kader. All this necessarily presupposes a numerous population, consequently also a favourable climate; and hence it results that the extension of our conquests in the south of Algeria is constantly revealing to us productive districts; for, up to the present time, we have scarcely passed 35° N., which is still very far removed from the region deserving the name of desert.

The coast of the Atlantic is not entirely free from the nullity of rains of the middle system which we are defining; for round Cape Barbas, from 22° to 21° N., two consecutive years sometimes pass without the smallest trace of rain; and the same is the case in the Cape de Verd islands, which are sometimes deprived of rain for a period of even seven or eight years.

To the east, between the Nile and the Red Sea, there extends the chain of the Mokattam, which may be considered as the line of the disjunction of Asiatic monsoons and the trade-winds; upon it, in consequence of its summits, and of the phenomenon of the monsoons, there are as violent and frequent winter rains as in Palestine, for they cause the mountain torrents to swell to overflowing; but this result is not to be regarded as contradicting the general law, for close to Qôceyr (lat. 26° 7′ N.) the sky is constantly serene, and no house possesses a cistern, although the nearest spring is distant a day's march. On the opposite side from the Red Sea, Thebes (lat. 25° 43′ N.) receives rain only during a small number of days in the year; it then ceases in Upper Egypt, but recommences in Dongolah in 20° N., where it becomes estival. There, also, the arid soil and the naked rocks cease, as well as the cloudless sky of Upper Egypt and Lower Nubia; but nevertheless, the pluvial intermission is still distinctly displayed in Dongolah; according to some, Gerry, in lat. 16° 15′ N., and according to others, lat. 17° N., is to be regarded as the true limit of the regular intertropical rains of that portion of the continent.

Various oases, or depressions of the surface (the oasis of Siwah is 100 feet below the level of the sea), are scattered here and there in the midst of the sand, and owe their fertility to springs. Among these there is one which should fix our attention in a particular manner, because it forms a sort of diaphragm, so to speak, dividing the Sahara into two equal parts, and serving as the great highway for the caravans from Fezzan to the centre of Africa. At the epochs of the year when the temperature is lowered, that is to say, from October to February, the caravans of the north, of the east, and of the west, meet at Mourzouk; they take fifty-seven days' march to reach Birney (the capital of Bornou, to the west of Lake Tchad, in lat. 16° N.), following a chain of rocks more or less abrupt, that enclose a sort of long valley (Wady Kawas), in which there are numerous stations, and consequently likewise that watery subsoil, without which the journey would be impossible. This line is only interrupted here and there by saline tracts, and by the sandy desert of Timtuma; and it is, therefore, not astonishing that it served as the foundation of commerce, and of the Carthagenian power in Africa, and that the English have endeavoured to secure its investigation by their alliance with the Pacha of Tripoli. On the side of Algeria, the route of Timbuctoo, by the oasis of Touat, is much less convenient; the stations are still rarer on the routes from Tafilet to the same town. It results, moreover, from the information obtained by Consul Jackson, that the oases there are liable to lose their springs,—a fact confirmed by the terrible example of 1805, when a caravan of two thousand persons and eighteen hundred camels perished entirely, from having trusted to these subterranean waters.

But this digression must not make us forget our principal object. The mountains of this median portion of the Sahara do not exercise a very sensible influence on the phenomenon of rain. Thus, in those which are to the west of Mourzouk, rains are so irregular that nine years sometimes elapse without their occurrence; the rough and rugged district of Haroudje alone exhibits valleys of bright verdure on account of the frequency of rain, but we must not forget that it is situated in 27° N. According to some travellers, rain is unknown even at Mourzouk (lat. 25° 54′ N.); and the small quantity of water which falls in the whole of Fezzan is so subject to intermission, that it cannot be depended on for the cultivation of the soil. Some garden products and corn are obtained in December and January only by the assistance of these springs; and Captain Lyon only saw three springs which reached the surface, the others being found at the bottom of holes dug to a depth of from three to seven yards. The oasis of Fezzan thus presents nothing else but a plain, generally sandy, steril, and destitute of streams worthy of remark; and this physiognomy continues to Tegerry (lat. 24° 4′ N.), where we have, at the same time, the southern limit of the date, and the northern of the *Cucifera thebaica*, and where, likewise, the rocky band already described commences.

Let us now place the facts relative to the opposite hemisphere beside those regarding the Sahara. The sub-tropical portion of Southern Africa presents to us, between the high regions of the centre of this continent and the mountains of the Cape of Good Hope, another series of deserts placed in a symmetrical position with reference to the Sahara. In fact, the coast of Cimbasia, to the north of the Orange river, as far as 19° S., did not present to the English expedition sent in 1824 any place susceptible of cultivation, or which was not even too dreadful for criminals. The small traces of very spare verdure, the brackish taste of the water of the streams, and the extreme scarcity of every other kind of water, indicate but too distinctly the absence of rain in that inhospitable region. In the interior, between the countries of the Namaquas and Damaras, some mountains and water-courses, constituting oases, vary a little this solitude; and then comes in the central portion a *terra incognita* for positive meteorology, where, however, the immense deserts of Tschallahenga and of Kalahari are indicated, forming in some measure the northern prolongation of the ochrey argillaceous Karoo grounds, a sort of steppes, whose soil is indurated during the nine months of dryness of their latitude. They are again interrupted in their approach to the oceanic coast by the extension of the mountains of Caffraria towards Monomotapa; but on the eastern side of these mountains, there are the low plains of Inhambana and of Sofala, and there it not only rains as little as in Lower Egypt, but the inhabitants of the Rio del' Agoa have never even seen rain fall.

In this continent, therefore, the phenomena are identical on both sides of the equator, and the first causes are consequently the same; let us, then, pause a moment, and cast a glance over the general configuration. It combines a very simple internal structure, with an outline devoid of articulations, and of notable denticulations; for, according to the observations of Russegger, the whole surface affects a very gradual ascending slope, from the north to the plains of Kordofan, which, although separated by an interval of 18° of lat. from the nearest shores of the Mediterranean, have only an elevation of about 1200 feet around El-Obheïd (lat. 13° 12′ N). This inclination becomes greater between 16° and 13° N., but without forming either terraces or other escarpments; and the plane of slope continuing in the same direction as far as 10° N., where it attains an elevation of from 1600 to 2100 feet, presents a vast plain, interspersed with mountain crests, elongated in various directions, unconnected with one another, and resembling large islands disseminated over the surface of the ocean. But to the south of Darfour and of Kordofan, in the country of the Gallas, the Dingas-Schillucks, and the Fungis, these asperities cease; and, as far as the eye can reach, nothing is to be discovered but a few rounded elevations, disseminated in the midst of the uniformity of the immense savannahs, which commence about 16° N. with the region of the periodical rains of the tropic. From 9° N., our information regarding central and southern Africa only permits us to suppose, that, in that part, the plateau continues to rise for a long distance, till it reaches an altitude of nearly 6200 feet, and that it afterwards descends rapidly, on the side of the Cape; so that, hitherto, no continuous chain, comparable to the chains of America, of Asia, or even of the Alps, has been found on the longitudinal axis of this inclined plane.

At the two opposite extremities of this continent, we have, on the one side, the Mediterranean mass of the Atlas, rising to the height of perpetual snow in Morocco, and sinking gradually to the east towards the plateau of Barka; and, on the other side, the mountains of the Cape of Good Hope and the Snowbergs, to the north of Camdebo, as high, perhaps, but, at all events, cold, and placed in 30° S., just as the preceding, which occur in 30° N.

From these terminal masses emanate, in some measure, the chains and the terraces of the east and west of the continent. The first (the eastern) extend from Caffraria, by Monomotapa, Mozambique, and Zanguebar, into the country of the Gallas, above which the elevations of Abyssinia rise to a height of from 10,000 to 14,500 feet; and they afterwards terminate towards the Mediterranean in the interrupted chains of Mokattam.

The band of western eminences forms, in the same manner, as regards Abyssinia, the mountains of Upper and Lower Guinea, the great mass at the sources of the Niger, the Gambia, and the Senegal, as well as the prolongation of these branches on the borders of the Sahara. According to Mollien, the altitude of some of these summits is such as to reach the limit of perpetual snow.

There results, then, from the position of the plane of general slope of this continent, combined with that of the fractures and rugosities of the coasts, a sort of immense central valley, which, commencing near the southern point of Africa, opens to the north upon the broad low plain of the Sahara; but the altitudes scarcely appear to be such as to perform any other part but that of an *épanouissement* in the regulation of the rains; the form of the plateau may accord with the physiognomy of the desert: the two intumescences of the east and of the west might alone occasion greater disturbances; but their opposite position, on very neighbouring parallels of the equator, is such, that, by favouring the atmospheric affluence of the two heteronymous poles, they only contribute to the establishment of the trade-winds, and consequently to the symmetry of the zones without rain and of the deserts.

The structure of the New World differs materially from the preceding; the essential forms no longer correspond, either in a parallel manner or symmetrically, with reference to the equator: the arrangement of the principal masses is even at right angles; the meteorological phenomena likewise are no longer everywhere identical.

In fact, for the east and west protuberances of Africa, situated in the neighbourhood of the equator, there is substituted the vast depression of the basins of the Amazon and Orinocco, to which the entrance of east winds is prevented, for a certain distance, by two mountain masses: the one, the Cordillera of Parima or of the Guianas, is comprised between the Orinocco, the Amazon, and the mouth of the Meta, and is composed of a mass of mountains, among which the Sierra of Duida attains nearly the height of the St Gothard; the other forms the Sierras of Amanbahy, of Mar, of Montequerra, of Vertentes, of Epinhaco, &c., which cover a portion of Uruguay, of Entre-Rios, of Corrientes, of Paraguay, and of Brazil, and comprehends, from the mouth of the Plata to Cape San-Roque, an extent of 30° lat. This vast plateau, having a mean height of about 2600 feet, on which are elevated chains presenting bold and sharp forms, whose summits have an altitude of about 6300 feet, offers, as the most characteristic feature, the mountainous band which extends from Rio Janeiro to near Pernambuco. This band rises on the Atlantic coast to a mean height of 3250 feet, and includes the lofty summits of Itacolumi, Itambe, and the Morro; so that if it constitutes between 23° and 10° S., a barrier analogous to that of the Guianas, it also differs from it in another respect, in that while it does not approach the equator nearer than 10° S., the other terminates at the equator.

It results, therefore, from the opposite position of these two littoral chains, that the mean axis of the largest of the basins of South America, that alone which traverses almost entirely the continent in a direction parallel to the equator, is, as it were, *refoulé* at 5° of south latitude, and that the breadth of its entrance on the Atlantic side is comprised between 0° and 10° south.

These masses of mountains become lower towards the interior, and lose themselves, the one in the Llanos of the Ornocco, and the other towards the Campos Parieceys, as well as in the vast plains of Moxos and Chiquitos, where it constitutes, in 16° and 18° south, a simple threshold, so to speak, upon which we may unite artificially the Paraguay to the Amazon by means of the Madera; just as on the other side in 2° and 3° N., the communication with the Orinocco is effected by nature by means of the Rio Negro, and the Cassiquiare. We have, therefore, a great system of depression, varying, according to Von Humboldt, from 200 to 1100 feet in height, extending from the Cordillera of the coast of the Venezuela to the Straits of Magellan, and forming an area of 456,900 square leagues. It comprehends the Savannahs, the Llanos, the Pampas, and the Steppes of the Orinocco, the Amazon, the Plata, and of Patagonia, which preserve, for distances of from twenty to thirty days' journey, an imposing and melancholy uniformity; palms grow at their one extremity, while the ground is frozen at the other; lastly, from time to time, this monotony is broken by lagoons, by sands, and by the mass of virgin forests, in the midst of which are discovered immense low islands of naked rock, rising only a few centimetres above the rest of the plain. We may next remark, that, for the deep Gulf of Mexico, placed between 10° and 30° N., we find substituted, in the opposite hemisphere, the broad enlargement and the mountains of Brazil; that the emaciation of form towards the South Pole is the substitute for the great transversal extent in North America; and, lastly, we may notice, as another character of want of symmetry, the arrangement of the royal Cordillera of snows. It approaches so near to the Pacific Ocean, that the space on the western side is scarcely worthy of the name of plains. For its fantastic configuration in South America, there is substituted a uniform plateau in Mexico; the chief routes there attain altitudes greater than the height of Mont Blanc; populous towns there exist at the level of the Col du Géant and of Mont St Bernard; and, lastly, the principal summits rise above the whole to a height of from 3000 to 6500 feet; so that, *a priori*, these immense heights, with their elongation from north to south, would seem to have caused to turn, at a right angle, the meteorological system, which, in Africa, corresponds with the direction of the equator. It thus becomes a matter of high interest to examine the disturbances which such a discordance can produce in the regularity of the great atmospheric phenomena, and to ascertain in what degree the effects of pure and simple solar action are modified by orographical circumstances. If we still find some trace of symmetry in the position of the deserts, the influence of a very energetic cause must be recognised in this trace, since it must have subsisted in the midst of so many causes of anomalies; and it will thus furnish us with the best proof of the law whose existence we are endeavouring to establish.

In the latitude of the Sahara, on the coast of the great ocean, the low lands of Old California are destitute of rain; the mountains which constitute the ridge of this peninsula alone receive a small quantity, and the vegetation is there as poor as the water is rare; so that, in this respect, the resemblance between that portion of America and the corresponding region of Africa could not be more complete. Nevertheless, some special phenomena seem to characterise these latitudes; for the atmosphere, almost constantly clear, is always of a deep blue colour; but if some clouds occur at the setting of the sun, they are ornamented with the most beautiful tints of green, purple, and violet, so that the scene then possesses an extraordinary beauty. If to this we add, that rain, during a serene sky, is pretty frequent in the Gulf of California, we shall perceive that there is a series of effects derived from a particular state of the aqueous vapour dissolved in the air, whose more minute examination is well worthy of the attention of navigators.

To the east of Lower California, and beyond the break in the continuity of the land, produced by the Gulf of California, there rises the vast undulated plateau of Mexico, which sinks rapidly on the opposite side in Cohahuila and Texas, to which succeed the low land of New Orleans and the Floridas, washed by the warm waters of the Mexican Gulf. These varieties of configuration must necessarily produce abrupt variations in the climate; and these affect the transition of the hemi-annual rains of summer into those of winter; so that a certain degree of attention is necessary to follow these different arrangements, and the following are the complications, whose existence observation has enabled us to ascertain.

According to the information communicated to me by M. Duport Saint Clair, the district of Cinaloa, situated opposite to Old California, possesses estival rains, which become rarer towards the north in Sonora, where they are very feeble, and very irregular. Beyond Guaimas, in lat. 28° N., a week often passes without any thing falling but a trifling shower; but this season is prolonged until December, when one or two tropical rains take place, which cause the rivers to overflow, and from that time these rains cease completely until June; in Sonora, between 27° and 32° N., the air is said to be generally healthy and salubrious, excepting on the coast; further towards the north, there are the fogs and irregular rains of Monterey, in New California.

On the plateau of New Mexico, in the same latitude as Central Persia and Syria, there are very intense colds; snow is sometimes seen to fall at Mexico in lat. 19° 25′ at the height of 7400 feet; nevertheless, this circumstance does not destroy the arrangement of the summer intertropical rains, although this arrangement becomes complicated by the winter subtropical rains. Thus, at Guadalupe-y-Calvo (lat. 26° 5′ N.), independently of the trimestrial estival rains of June, July, and August, which prevail at Mexico, there is a repetition from October till January, during which there are two or three falls of very cold water, continued during several days, and to which the inhabitants of the country give the name of *equiparte*. We see, then, here, the reproduction of the phenomenon of Sonora; so that the hiatus of the Sahara is awanting in this region, and, in fact, meteorology has acquired a knowledge of various modes of transition from one climate to the other, of which it was ignorant until lately. But these rains do not entirely efface the droughts; for, according to Von Humboldt, the latter are frightful in a portion of this region, where, moreover, a specimen of a desert is found in the arid and dry plain of Muerto, the extent of which is about thirty leagues; the dews which are so abundant in Sonora do not exist on those heights. At Chiahuahua, in 25° N., the seasons are still less marked by irregular rains; and, the atmosphere is so destitute of aqueous vapour, that, at night, during a bivouac, the mere touching one's coverings is sufficient to produce electric sparks, and a Leyden jar can be charged in this manner.

On the eastern side, Xalappa (lat. 20° N.), at the height of the *tierras templadas* (temperate regions), is frequently enveloped in fogs, and there are frequent rains there at all seasons; but from Stander to Monterey, from 23° to 26°, and on the banks of the Rio-Bravo-del-Norte, as far as Texas, we immediately find, as in Algeria, the winter rains characterised by small falls of snow, and by the north wind. In this last country, where there are vast savannahs, they are accompanied by frightful storms, while, in the first, there are some summer rains. From this there result transitions analogous to the preceding; but the warm waters of the gulf, as well as the coast of the Atlantic, increase the various causes of anomalies; for, at New Orleans, in lat. 29° 27′, rains fall during the whole year, although the most violent are in summer; whereas, at Heywest, the most southern town of the United States in 25°

N., there are, as in the south of France, rains of spring and of autumn intermixed with those of all seasons.

To recapitulate, the band of droughts and of deserts is only indicated in this portion of the northern hemisphere by the stations of Lower California, leaving which, it is, first of all, effaced by the junction of the estival rains with the hivernal rains, and is afterwards completely obliterated on the coasts of the Gulf of Mexico; but it suffices for shewing the regularity of the law, that it is distinctly indicated at some points, and we ought only to regard the disturbances as resulting from the influence of the configuration and the relief of the surface, the effect of which we also endeavour to appreciate.

The southern portion of the New World exhibits other examples of the same circumstances,

(*To be continued.*)

Eighth Letter on Glaciers. Addressed to PROFESSOR JAMESON by PROFESSOR FORBES.

Experiments on the Plasticity of Glacier Ice.

GENEVA, 30*th August* 1844.

MY DEAR SIR,—The theory of glaciers has now reached that point when it can only receive some material addition by the multiplication of accurate measurements; and these measurements must be conducted in the manner which will best discriminate between rival hypotheses, and, if possible, yield *direct* instead of indirect proofs of each fundamental fact assumed. In my former letters I have insisted sufficiently upon the importance of the results which a system of nice measurement has introduced into this branch of science, and their value to the theorist who afterwards wishes to put numerical for unknown quantities in his investigations; I also shewed that there is a continuity and approximate constancy in the motions of glaciers, which permits us to obtain, with certain precautions, in a few days, better results than any one had previously acquired during the lapse of months or years. I have now to announce to you that I have pushed these measurements to a still greater degree of minuteness, and with results which shew that the methods I have employed are trustworthy, and are able to afford the direct solution of questions which at first appeared to admit of only indirect or inductive proof.

Of this class by far the most important appeared to be the manner in which the glacier alters its form in such a way, and to such a degree, as to suffer its central portion to descend towards the valley with double or treble the velocity of its lateral parts. Such, for instance, I have found to be the case in the middle region of the great glacier of Aletsch, where its inclination is small (about 4°), and where the continuity of the ice with the side wall is preserved without the interference of large fissures. I there found that, whilst the velocity of the ice at 1300 feet, or about a quarter of a mile, from the side, is 14 inches in 24 hours; at 300 feet distant from the side it was but 3 inches in the same time; and, close to the side, it had nearly, if not entirely vanished. Facts like this seem to shew, with evidence, what intelligent men, such as Bishop Rendu, had only supposed, previously to the first exact measures in 1842, that the ice of glaciers, rigid as it appears, has in fact a certain "ductility" or "viscosity," which permits it to model itself to the ground over which it is forced by gravity,—and *that*, retaining its compact and apparently solid texture, unless the inequalities be so abrupt as to force a separation of the mass into dislocated fragments, such as it is well known that every glacier presents, when the strain upon its parts reaches a certain amount,—as when it has to turn a sharp angle, or to descend upon a rapid or convex slope.

The mutual action of the parts of the glacier, the drag which the centre exerts upon the sides (and, by an exact parity of reasoning, the top upon the bottom), seemed to me so obvious, after measurement had proved their variable velocity, and observation had shewn that this was not necessarily accompanied by a general dislocation of the mass,—that I should scarcely have thought of attempting a direct proof of the yielding and ductile nature of glacier ice, had I not been favoured by Mr Hopkins with copies of his two ingenious papers on the subject of glaciers, read to the Cambridge Philosophical Society on the 1st May and 11th December 1843, which were put into my hands here less than a month ago, by his friend Mr Williamson. I there found it stated that there is "a necessity of proving, by independent experimental evidence, that glacier ice does possess this property of *semi-fluidity* or *viscosity*, if we would attribute to that property the effectiveness of gravity in setting a glacier in motion."—*First Memoir*, p. 3.

Since Mr Hopkins admits the fact of the swifter central motion of the glacier, he must have recourse to some mechanical explanation of the fact. This he does by assuming the existence of vertical fissures, parallel to the sides of the glacier, dividing it into a series of longitudinal stripes, whose adjacent surfaces, according to him, slide over one another, and, in the case of a glacier forcing its way through a gorge, the lateral portions are altogether arrested, whilst the central parts slip down between them.

These parallel stripes of ice are supposed by Mr Hopkins to be of considerable breadth, and to have no sort of analogy with the ribboned structure, to which the readers of my earlier letters will recollect that I have ascribed a similar origin, being lines of discontinuity arising from the crushing of one portion of the semi-rigid glacier past another. This Mr Hopkins regards as "no more possible than that a mass should permanently maintain a position of unstable equilibrium." The veined structure of glaciers he considers to be unexplained, and, in the present state of science, inexplicable.

Although the general absence of such a system of longitudinal fissures as Mr Hopkins has figured in page 14 of his First Memoir, and the regularity and continuity of motion of the glacier and of its parts, wholly inconsistent with the jostling of huge masses of dislocated ice, might be considered as a sufficient answer to this modification of the theory of De Saussure, the consideration of this demand for a direct proof of the flexibility of glacier ice led me to think of its practicability; and I shall now state what I have succeeded in doing, towards the solution of this practical question in the only way in which it admits of being treated, namely, by the assiduous observation of the motion and change of form of a small compact space of ice on a glacier. The Mer de Glace of Chamouni offers fewer fit points for such an experiment than many other glaciers, since in all its middle and lower portions the ice is excessively crevassed near the sides. There is one spot, however, between the "Angle" and Trelaporte, below the little glacier of Charmoz, where the ice is extremely flat and compact for a space of about seventy yards in width, and several hundred yards in length, which is wholly devoid of open crevasses, and where I expected to find the variation of velocity from the side towards the centre very sensible, because the veined structure is there more perfectly developed than in any other part of the glacier. In this anticipation I was not disappointed. The ice in question is separated from the western moraine of the glacier by a space deeply crevassed 50 or 60 yards wide. The entire breadth of the glacier is here at least 800 yards. The central part has great transversal crevasses due to the rapid descent of the glacier where it sweeps round the promontory of Trelaporte immediately above. There is no trace of longitudinal fissures of any kind, except the true blue veined or ribboned structure, which, as already mentioned, is here exceedingly developed; giving to the even part of the glacier already specified the appearance of exquisite veined chalcedony of an aqua-marine colour; and the vertical plates of ice thus subdivided are so distinct as to produce a *true cleavage* when the ice is broken by a hammer or cut with an axe. When the glacier is wet, the blade of a knife may be introduced to a depth of some inches between the laminæ, which are commonly not more than a quarter of an inch apart.

I fixed in a line transverse to the axis of the glacier six stations. Over the first of these the theodolite was regularly centered, in order to observe the relative motions of the others which were respectively 30, 60, 90, 120, and 180 feet distant. Finding that, even in the course of a single day, the acceleration of the more central parts was evident, and the six points in question formed a portion of a continuous curve, I subdivided the first 90 feet from the theodolite into 45 spaces of 2 feet, each of which was marked by a perforation in the ice into which short pins could be accurately fitted, and the *deformation* of this straight line of 90 feet in length was carefully observed at short intervals. The errors of the original places of the marks were determined by a simple but nice process, and their daily progress was similarly noted. I have now before me the registers and also the graphical projections of the actual places of this portion of the curve of flexure of the ice, cleared of the errors arising from the movement of the theodolite, which was itself placed upon the ice, which error was independently determined. You will probably be surprised when I state, that in seventeen days, the part of the glacier 90 feet nearer the centre than the theodolite, had *moved past* the theodolite by a space of 26 inches, and the intermediate spaces in proportion. When I was reluctantly compelled to cease my observations on the 45 marks, they had, in the course of six days, formed a beautiful curve slightly convex towards the valley; and as the vertical wire of the theodolite ranged over them, their deviations from a perfect curve were slight and irregular, nor was there any great dislocation to be observed in their whole extent; proving the general continuity of the yielding by which each was pushed in advance of its neighbour. During these six days the 45th mark had shifted 10 inches; and besides this obliquity of the line of pins (=31′ 46″), they had a convexity whose versed sine was about an inch. All this, viewed in prospective with the theodolite, left no remaining doubt as to the plasticity of the glacier on the great scale.

Lest, however, the convexity should have been too small, in so short a time, to admit of measurement, I had provided another test, in order to shew that the progressive advancement of the line of marks was due to the actual deformation of the ice, and not to the mass of the glacier in this part revolving round some fixed or moveable centre. For this purpose, I fixed a mark in the glacier, 20 feet from the theodolite, and in a direction perpendicular to the before mentioned line of marks. It was, therefore, seen from the theodolite in the direction of the length of the glacier, and, consequently, was not liable to displacement by its motion. I measured, from time to time, the angle between this mark and the several marks transverse to the glacier, and I found that this angle became continually, and without any exception, more and more obtuse. During seventeen days, it revolved through an angle of about a degree and a half.

I reserve to another opportunity the publication of the details of the measurements and the graphical projections, which offer, when minutely examined, some interesting peculiarities too long to specify. The main conclusion is, that even the most compact parts of the ice yield to pressure, and that where no fissures exist, there is a sliding of the parts of the ice over one another, or else a plasticity of the whole mass. With the abundance of blue bands before us in the direction in which the differential motion must take place (in this case sensibly parallel to the sides of the glacier), it is impossible to doubt that these infiltrated crevices (for such they undoubtedly are) have this origin, and are the main mechanism of the forward motion; but it occurred to me, on one occasion (the 23d August), to obtain all but ocular evidence of the fact. Standing at the theodolite with an assistant, we heard a dull noise in the ice within a very few feet of us, attended (I think) with a slight tremor, and followed by a rushing and hissing sound. As we were very near the great crevasses of the moraine, it was, no doubt, a subsidence of a portion of the glacier, and the rushing was occasioned by the more rapid flow of the superficial streamlets in the direction of increased inclination of the ice. I instantly searched in all directions, but in vain, for the slightest evidence of the fracture of the ice. All that I could see was, that where the veined structure was best developed, innumerable air bubbles escaped through the superficial water, which was slowly imbibed in those parts where the strain had expanded the ice, and thus enlarged the capillary fissures between the blue bands.

Mr Hopkins has done me the honour, in the memoirs before alluded to, to mention with approbation my observations and experiments on the subject of glaciers. He has been more sparing either in praise or criticism of the theory which I have founded upon them. Had Mr Hopkins applied himself with equal care to that as to other parts of my writings, he would have observed coincidences in our views which he appears not to have noticed; and he would probably have hesitated before laying down so broadly as he has done, an objection to the Viscous Theory, very easily refuted, and some peculiar views which he considers distinctive of his manner of considering the subject, from De Saussure's and my own. I shall probably, on another occasion, endeavour to shew that, by following out his own principles, the results must inevitably merge in mine, when what is inadmissible shall have been subtracted.—I remain, my dear Sir, yours very truly,

JAMES D. FORBES.

P.S.—The influence of the Dimension, Slope, and absolute Elevation (or surrounding temperature) of glaciers upon their motion, is a matter of observation in detail which offers no peculiar difficulty, and which deserves to be extended. Having measured the rate of motion of perhaps the largest glacier in Switzerland (the Aletsch), I have also measured one of the smallest, a glacier of the second order, near the Hospice of the Simplon, almost 8000 feet above the sea, and not many hundred feet in length. The velocity was little more than an inch in twenty-four hours, a result corresponding with the extreme dryness of the *névé* at that elevation, indicated by the very trifling issue of water from beneath, and to the insignificant vertical pressure of so small a mass, notwithstanding its considerable slope. A similar result, it must be owned, might be expected in this case upon almost any theory.

Extract from a Letter from Rev. George B. Warren, to Dr Davy, relative to a Sooty Deposit on the Surface of the Sea, off the Coast of Devon.

Your paper in Jameson's Edinburgh Philosophical Journal on the carbonaceous deposit on the lakes of Westmoreland, recalls to my recollection a similar phenomenon which I have noticed on the sea off the coast of Devon. During a residence of five years at Sidmouth, I generally remarked, that after a calm of two or three days, the surface was covered with a deposit which had the appearance of very fine powder intermixed with soot. I at first thought it must have been occasioned by the dust and smoke from the town, but finding it equally diffused over a space of eight or ten miles, and at some distance from the shore (indeed there was every reason to suppose that it extended many miles in every direction), I was obliged to look to some other quarter for the cause of so singular an appearance. The absence of all large towns or manufactories in this part of England, induced me to suppose that the matter which so extensively covered the water, must have been conveyed by the winds from the smoke of London, and this opinion was strengthened by the fact, that on every occasion when I had noticed the phenomenon in question, the wind had for some days been blowing from the east. About three years since, I observed a magnificent water-spout cross from Torbay to the immediate neighbourhood of Sidmouth, and being very near the spot where it struck the land, I was enabled to observe that the column of fine spray raised by the vortex, reached fully the height of seven hundred feet, being at least an hundred feet above the top of the cliff. The column was travelling at the time a little north of east, and as the newspapers announced the fall of some small fish in a heavy shower of rain about half an hour afterwards in the streets of Salisbury, they were, no doubt, the small fry swept up with the surface water, and which were kept suspended in the air as long as the vortex lasted. It cannot, therefore, be inconsistent with probability to suppose, that the smoke of London may be conveyed to the coast of Devon by the east wind, and deposited on the sea as soon as the quiet state of the air should allow it to subside.

G. B. WARREN.

7. MONT LE GRAND, EXETER,
August 5. 1844.

On the Mammalia of the Counties of Aberdeen, Banff, and Kincardine. By WILLIAM MACGILLIVRAY, A.M., Professor of Natural History, in Marischal College and University, Aberdeen. (Communicated by the Author.)

The north-eastern portion of the Middle Division of Scotland forms an extensive natural district, of which the indigenous animals may be presumed to be not less worthy of examination than those of other parts of Britain. It is formed of the three counties of Aberdeen, Banff, and Kincardine, which, though presenting considerable diversity of surface, and differing from each other in various respects, unite so naturally, that in traversing them, one finds no very abrupt transitions either in a geological or a geographical point of view. Bounded on the east by the German Ocean, and on the north by the commencement of the Moray Firth, it stretches inland toward the central parts of the island, where mountain ranges of considerable elevation, on which are some of the highest summits in Britain, constitute a highland tract not surpassed in stern grandeur by any other in Scotland. Among these ranges of granite mountains are found the sources of the North Esk, the Dee, the Don, the Doveran, and some of the tributaries of the Spey. The first of these rivers forms the southern boundary of the district, while the last terminates it on the northern side. From the upper or more inland portion of the district, commencing with the Cairngorm, Ben-na-muic-dui, Ben-na-buird, and Loch-na-gar mountains, the high land declines eastward, sometimes terminating rather abruptly, but generally passing gradually into the comparatively level tracts of the south-eastern and north-eastern lowlands. The whole district, however, is essentially hilly, and composed of granite, gneiss, mica-slate, and serpentine, together with greywacke and old red sandstone. Massy, rounded hills, covered with detritus, scarred by the torrents, and partially clad with heath and coarse herbage, valleys in which green pastures and corn-fields alternate with wild woods and plantations; undulated plains, partly in their natural state, and partly converted into fertile fields; tracts of maritime sand, covered with bent, and thickets of furze, long ranges of sea-cliffs, often of great height, together with numerous streams, and a few lakes, afford fitting habitations for quadrupeds and birds, as well as reptiles, of which the number is few, and fishes, which, although not numerous as to species in the fresh waters, abound in the seas that wash so extended a portion of the boundaries of the district. Compared with the western coast of the middle division of Scotland, that of our eastern side is remarkably continuous, the sinuosities which it presents being but slight. For this reason, in part, the marine predaceous quadrupeds are of rare occurrence with us. But our terrestrial mammalia appear to be as numerous as in any other district of equal extent in Scotland; and some species rare in other parts are common with us. It seems inexpedient, however, to enter into geographical details, which may with more effect be introduced on occasion. I shall, therefore, proceed to give an account of the mammalia which I have met with, taking leave here to state that all the descriptions are from objects found in the district; and that, though my observations are not so complete as one might wish, they may prove interesting to those who have not themselves made a more extended examination, as well as to naturalists in general, to whom our district is not quite so well known, in a zoological point of view, as it deserves to be.

The Mammalia of Aberdeenshire, and the two neighbouring counties, belong to the orders of the Bimana, Chiroptera, Insectivora, Carnivora, Rodentia, Ruminantia, and Cetacea. Of the first of these orders, it seems inexpedient to say anything; for although the people of the district have many good qualities, physical and intellectual, they have also some defects, and an account of either would necessarily be viewed with less good-will than would prove agreeable.

ORDER CHIROPTERA.

Of the Chiroptera, characterised by having the anterior limbs much extended, and connected with the posterior by a bare expansion of the skin, so as to render them organs of flying, together with pectoral mammæ, we have only three species, belonging to two genera of the family of Vespertilionina.

Fam. VESPERTILIONINA.—Molar teeth with acute tubercles; anterior digit elongated, the first only being short and free, and having a claw, the rest connected by a broad membrane, which extends along the sides to the hind feet, and from them to the tail.

Gen. PLECOTUS.—Head roundish, forehead flat, with a bare longitudinal line; wings and ears very thin, the latter very large and elongated.

1. *Plecotus auritus.—Common Long-eared Bat.*
Ears more than twice the length of the head, oblong, rounded, their inner margin and longitudinal rib ciliate; tragus about a third of the length of the ear, lanceolate, rather obtuse; fur brownish-grey above, pale grey beneath.

This Bat has been found in the roof of Old Machar Cathedral, whence Mr Thomas Smith took several specimens, in the summer of 1841, five of which I saw, and of which one was given to me by him. Another specimen was obtained there in the summer of 1844; and I have seen or heard of a few more that were procured in different parts of Aberdeenshire.

Gen. VESPERTILIO.—Head oblong, forehead convex, muzzle rounded; wings and ears thin, the latter moderate, subovate.

1. *Vespertilio Daubentonii.—Daubenton's Bat.*

Ears ovate, obtuse, one-third shorter than the head, deeply sinuate on the outer margin, and having at the base a small rounded opercular lobe, convex on the inner margin; tragus nearly half the length of the ear, narrow, tapering, rather acute, slightly incurved, and having a small angular lobe at the base externally; cheeks tumid; space about the eyes rather bare; a small tubercle bearing a tuft of long hairs before the eyes; fur rather long, dense, soft, dull reddish-brown above, light brownish-grey beneath. Young, dusky above, dull grey beneath.

In seventy-two individuals examined, there were scarcely any variations in colour. Some were slightly lighter or browner above, some brownish-grey, silvery-grey, or light-grey beneath. In some the hair was longer, in some finer and more glossy. The tragus varied considerably in form; in some it was almost straight, in some with a sinus or slight notch near the end externally; in all the point tapering, but in some more obtuse than in others, never blunt and rounded, nor ever quite acute. In some the interfemoral membrane light-grey, almost whitish, in others dusky. The soles dusky flesh colour, lighter or darker.

This species is distinguished from the Pipistrelle Bat by its larger size and different proportions, but especially by the form of the tragus, which is much narrower, and not rounded at the end, but tapering to a rather obtuse point. Its hind feet are also much larger.

It is very abundant about Old Aberdeen, and is found in great numbers in Old Machar Cathedral, where its slumbers have not been much disturbed until recently. They retire for the season from the middle of October to the end of November, according to the temperature. In the winter of 1842, I have seen them flying so late as the middle of December. In spring, they sometimes appear by the end of February, generally about the middle of March.

On the 20th September 1841, accompanied by two young friends, I ascended one of the steeples of the Cathedral to make search for bats, which were represented as being of frequent occurrence in various parts of the building. Mr Thomas Smith, one of my companions, having apprised us of a favourable place under the roof, we slipped over the bartisan upon the slates, and obtained ingress. The beams, placed at a considerable distance from each other, with the intervening spaces occupied by very thin boards, afforded rather dangerous footing; but, having lighted our candles, we proceeded cautiously. Having advanced a considerable way into the dark space, my companions ascended upon the cross-beams, when one of them, my son, discovered a conglomeration of bats, clinging to the roof and to each other, and Mr Smith found another not three feet distant. They presented a very singular appearance, as they stuck together like clusters of bees, and excited the admiration of us all. A very large proportion was consigned to two handkerchiefs; but probably a third at least escaped. A smaller group, of about ten individuals, was afterwards discovered by me, and secured by Mr Smith. In this part of the building quantities of their dung, almost incredibly large, were observed on the beams and elsewhere; and several of the upper steps of the stair of one of the steeples were thickly covered with it. The quantity seen might be rudely estimated at two barrowfuls. On returning, we let loose the bats in my working-room. The number, as we afterwards ascertained, was seventy-four, of which about forty were liberated or escaped.

These bats are very active. They hobble along the floor with considerable speed, and can rise on wing very readily from a flat surface. Their flight is moderately quick, and usually silent; but in hovering, when intending to settle, or in turning a corner, or when any impediment presents itself, the flutter of their wings may be distinctly heard at the distance of twenty or thirty feet. They utter a feeble, shrill, rather harsh cry, and when irritated, emit an incessant querulous chatter. On being seized, they generally try to bite, some individuals shewing considerable ferocity, while most are timid. When single, they feel cold to the hand, especially their membranes; but when many are together, a most surprising degree of heat is generated. Forty of them kept some time in a large tin cannister, rendered it quite warm to the hand applied to its lower part externally. The moisture caused by their perspiration, or otherwise, covered the sides and top in drops, and ran down as on the glass of a window in damp weather, and their hair at length became soaked with it.

In the middle of October of the same year, I revisited the steeples and roof, accompanied by Mr Leslie and my son. The weather was boisterous and cold, there being a fierce easterly gale, with rain. Not a single individual could be found. They had probably retired to their winter quarters, at least temporarily.

This species is much infested by parasites of three kinds. At least, I have seen individuals of which the membranes were dotted all over with inflamed spots caused by their punctures; and some, which were found in the churchyard unable to fly, seemed to have been reduced to that state of debility by these animals.

I have frequently seen it flying in the evening, even before sunset, and, on a few occasions, early in the morning; but until the autumn of 1843, had not observed it abroad at midnight. In clear moonlight nights, however, it flies all night, as probably do all the other species.

In so far as I know, this species has not hitherto been observed in any other part of the district; but there is reason to think that it is extensively distributed, as bats are not uncommon in very many places.

2. *Vespertilio Pipistrellus.* Pipistrelle Bat.

Ears ovato-triangular, a little shorter than the head, sinuate on the outer margin, and having at the base a small rounded lobe, nearly straight, on the inner margin; tragus about half the length of the ear, linear-oblong, slightly incurvate, rounded at the end, and having a sinus on the outer margin near the base; cheeks tumid; space about the eyes rather bare; a small tubercle bearing a tuft of longish hairs above the eyes; fur rather long, dense, soft, deep reddish-brown above, of the same tint, or pale beneath; young dusky above, not much lighter beneath.

Of the habits of this species, as observed in Aberdeenshire, I have nothing to say, it being impossible to distinguish it on wing from Vespertilio Daubentonii. In 1819 I found a specimen in Corby Den, Maryculter. Mr Leslie has in his collection a specimen said to have been found in Aberdeen. I have also seen it at Peterhead and Banff.

ORDER INSECTIVORA.

The Insectivora have the feet adapted for running, sometimes also for digging and swimming; several small molar teeth; large molar teeth with angular points; incisors, if present, thin-edged; canine teeth large, oblique, generally compressed. Six species have been observed with us; one belonging to the Erinaceina, four to the Soricina, and one to the Talpina.

Fam. ERINACEINA.—Two large canine teeth above, two below, without incisors; several small molar teeth, and several larger, with sharp tubercles, in both jaws; limbs short, all five toes; claws strong, arched; tail very short; body covered above with prickles, and capable of being contracted into a globose form.

Gen. ERINACEUS.—Head oblong, muzzle pointed; eyes of moderate size; ears broadly rounded; anterior claws large.

1. *Erinaceus Europæus.* European Hedgehog.

Ears very short, rounded; prickles of moderate length, yellowish-grey in their lower half, brownish-grey in the upper, with the tip pale; lower parts with yellowish-grey stiff hairs, mixed with brownish-grey woolly hairs.

Although twenty years ago of very rare occurrence, or confined to particular tracts, the hedgehog is now generally dispersed over the district, being found in all the lower parts, from the coast to the highland valleys of the interior, in many places in great abundance. It is especially plentiful along the Dee, as at Ballater, Banchory, and about Aberdeen, as well in the parishes on the Don. In some parts of Formartine it is also abundant, and of late years has extended, more or less, over the greater part of Buchan. In the New Statistical Account of the Parish of St Fergus, it is stated that, about 1834, before which it was not known in the district, it was discovered in St Fergus, on the farm of Nether Hill; and in that of Banff it is observed, that, of late years, several instances of its occurrence in the parish have been known, although it was unknown there at no distant period. It is probable enough that the alterations which have taken place in the country, and especially the increasing shelter of woods, may have given rise to this extension. With us it frequents thickets and other sheltered places, reposing by day in some crevice or concealed corner, or among whins or other shrubs, and searching by night for its food, which consists of slugs, snails, worms, larvæ, and insects. In the beginning of winter, it forms a large nest of leaves and grass in some sheltered and concealed place, and enters into a lethargic state, which continues until about the end of spring, or earlier, according to the temperature, when it leaves its retreat, generally much emaciated.

The form of its skull and teeth shews that it closely approaches to the Shrews, which belong to the next family, and both groups present a considerable resemblance to the Insectivorous Chiroptera in these respects.

Fam. SORICINA.—Two large, more or less decurved, canine teeth above, two more elongated, generally protended, canine teeth below; several small, compressed, anterior, and large tuberculate molar teeth in both jaws. Anterior limbs very short, with the feet rather long; posterior limbs short, slender, with the feet narrow; the claws compressed, slender, acute; body sub-cylindrical, or somewhat tapering behind, with fine soft or velvety pile; tail of moderate length.

Gen. SOREX.—Head depressed, tapering to a long narrow muzzle, with the snout small and mobile; nostrils small, terminal; eyes very small; ears very short, broadly rounded; mouth small; in the upper jaw, on each side, a large, more or less decurved, lobed canine tooth. from three to five small, and four large, many-pointed molar teeth in the lower jaw, on each side, a very large canine tooth directed forwards; body nearly cylindrical, or somewhat tapering behind; neck short; tail slender, of moderate length; anterior limbs very short, with the feet slender, five-toed; posterior limbs short, with slender toes; claws small, arched, compressed, very acute.

The British species of this genus have received much attention from Mr Jenyns, to whom we are indebted for nearly all the correct knowledge hitherto possessed respecting them. I have made those of our district very special objects of study, and have some hope that the following minute descriptions of them will be found not uninteresting:—

1. *Sorex Tetragonurus.* Square-tailed Shrew.

Dusky-brown above; reddish-brown on the sides; greyish-white beneath; feet very small, with short hairs; tail half the length of the body and head, four-sided, narrower at the base, of nearly equal breadth throughout, depressed toward the end; feet beneath, covered with short, adpressed hairs, forming a pointed terminal tuft; upper canine tooth bilobate, with the lobes obtuse, the anterior little longer; first and second small molar teeth nearly equal, and level with the basal lobe of the canine tooth; third and fourth much smaller and nearly equal; fifth very small; lower canine tooth directed forwards, with three, generally faint lobes on the edge; teeth tipped with brownish-red.

This, our most common species, may be more fully described as having the body sub-cylindrical, and rather full; the head oblongo-conical, somewhat less than a third of the length, excluding the tail, which is nearly a half of the whole length, including the head; the snout long, tapering, somewhat square, with a deep broad groove above, and a narrow groove beneath, mobile, and projecting far beyond the jaws, with the bare tip narrow and emarginate; the ears short, rounded, with two large thin internal lobes, the upper and outer parallel to the margin; the eyes very small, convex; the eyelids forming a narrow elliptical aperture; the feet short and small; the anterior with the first toe much shorter than the fifth, the second considerably shorter than the third and fourth, which are equal and longest; the sole bare, with six tubercles and numerous small prominences; the claws rather long, compressed, little arched, acute;

the hind feet longer, with the first toe a little shorter than the fifth, the rest nearly equal, but the second shorter, the claws more slender, the sole bare, with seven tubercles and numerous papillæ; the tail four-sided, narrower at the base, afterwards of nearly equal breadth, depressed toward the end, scaly, with short, adpressed hairs, the terminal forming a stiffish point.

The fur is soft, close, somewhat velvety; on the upper parts dark reddish-brown, or dull brown, sometimes intermixed with whitish hairs; on the sides reddish-brown or light greyish-brown; beneath greyish-white, on the breast tinged with yellowish. The lower surface of the snout, and the lower lip, flesh-coloured. The feet pale flesh-coloured. The snout with long, spreading, extremely fine bristles. The feet not ciliated, but having short, adpressed hairs. The tail dusky-brown above, silvery-grey beneath and on half the sides.

Canine teeth $\frac{1}{1}$, anterior molar $\frac{5}{2}$, molar $\frac{4}{3} = \frac{10}{6} = 32$.

In the upper jaw, on each side, the canine tooth bilobate, obliquely compressed, obtuse, the terminal lobe considerably elongated, decurved in about the eighth of a circle, obtuse, but thin-edged at the end, curved inward, the two teeth meeting near the end. First laniar tooth obliquely compressed, rounded, or rather pointed at the end, about the same size as the basal lobe of the canine tooth, or rather larger; second similar, about the same size, and equal in length; third much smaller, and less elevated; fourth a little smaller, fifth much smaller. First molar tooth large, with two anterior external, conical, rather acute prominences; the second larger, and a thin-edged ridge behind, terminating in a slight prominence in contact with the next; a little behind the anterior lobe internally is a small lobe, and nearly in a line with these two an internal less elevated lobe. Second grinder smaller, with three external lobes, two internal terminating the transverse grooves between the outer lobes, and two inner lobes. Third grinder with three external lobes, two internal, and two inner. Fourth grinder very small, transverse, with the crown irregularly concave, and a posterior prominent thin edge.

In the lower jaw, the canine tooth horizontal, rather slender, obliquely compressed, thin-edged, with three obtuse lobes on the margin, the last lobe less elevated and broader, the tip obtuse but thin-edged. The first laniar compressed, thin-edged, rising into an anterior thin, obtuse lobe; the second larger, but similar; the first grinder larger, with three external points, the middle largest, the anterior projecting inwards, and two internal points; the second grinder similar, but smaller; the third much smaller, but similar.

The teeth are white, with the tips brownish-red, or reddish-brown, sometimes blackish-brown. On the grinders of the upper jaw the red is chiefly on the inner, on those of the lower jaw on the outer side.

	MALE.		FEMALE.		FEMALE.		FEMALE.	
	Ft.	in.	Ft.	in.	Ft.	in.	Ft.	in.
Entire length, ...	5	3	4	10	4	9	4	8
Length of head, . . .	1	1	1	0	1	0½	1	0
Length of tail, . . .	1	10	1	7	1	7	1	6
Length of fore foot, . .	0	4½	0	4¼	0	4¼	0	4½
Length of hind foot, .	0	6¾	0	6½	0	6½	0	7
Skull in length, . . .	0	10	0	9¼				
Skull in breadth, . . .	0	5	0	4¼				

Individuals vary considerably in size and colour, as well as in the dimensions of the snout, and in the length of the hairs on the tail; they being in some worn at the tip, in others forming a pointed tuft. After the completion of the moult in June, the pencil at the tip of the tail is finely pointed, and some of the hairs project nearly a quarter of an inch. In an old female, obtained on the 30th of June 1843, the hairs on the tail were adpressed and very short, those at the tip obliterated. The upper parts vary from very dark-brown to chestnut-brown; the sides, although always distinctly of a lighter brown, also vary in tint; and the lower parts from brownish-grey to greyish-white. The teeth vary chiefly from being used. The lower canine, in old individuals, has its anterior lobes worn down, so as to represent only the two posterior lobes.

In an adult, the œsophagus was about an inch in length; the stomach ovate, five-twelfths of an inch long, and four-twelfths in breadth; the intestine eleven and a half inches long, and from one and a half to two and a half twelfths in breadth.

This species is common, and generally distributed in the three counties, being found equally in the lower districts and in the highland valleys. It occurs on dry grassy and mossy banks, in woods and copses, by hedges, on links or downs, and in fields, meadows, and pastures. It feeds on insects, larvæ, and worms, in searching for which it works its way in concealment, among the moss and herbage, forming long tortuous passages or galleries, similar to those of the mole, and which it appears to force more by its snout than feet, as the claws are seldom in any degree blunted. Individuals are often met with in hay fields, and even in wet meadow ground. But it is among the long tangled grass by walls and hedges, and especially among the ferns and thick herbage of rocky banks, that it is most numerous. Often, when it cannot be seen, its presence is made known by its shrill, weak, little modulated cry. On the surface it runs with considerable speed; but it is not there so active as a mouse, for it is liable to be tripped by the herbage. It is very often found dead in the woods and pastures, especially from the middle of May to the end of September, and, I think, more particularly in dry weather. It is difficult to discover the cause of this mortality; but, judging from circumstances, I should suppose it to be owing to continued drought, which either destroys the worms near the surface, or causes them to retreat to an inaccessible depth, for the runs of this species are not found to pass into the soil. But I have also seen shrews lying dead on paths in the woods in rainy weather. In most cases those found in such situations have been crushed by the foot; but frequently I could not trace any injury inflicted upon them. Owls and Kestrels frequently prey upon it; but cats, although they kill it, refuse to eat it, which may, perhaps, be owing to the peculiarly fetid odour that emanates from it.

Several foolish notions prevail respecting it among the country people, who allege that by running over the backs of horses it causes lameness in them, and that it is unsafe for a person to walk over it. Some imagine that it lames for life the foot over which it happens to run, and others are afraid of allowing it to pass between their feet. It is, perhaps, on this account that it is so generally killed when met with. The creature, however, is exceedingly harmless; and, as it feeds on worms and insects, it rather merits protection.

There is reason to believe that it becomes torpid in winter, it being never heard or seen during the cold months of December and January. Its periods of parturition, and its mode of nestling, are not known to me from observation; nor have I found any person who could give information respecting them. But a female, which I opened on the 30th of June 1843, contained eight young ones; and in one examined on the 14th July there were seven. These are positive facts, worth any number of conjectures. The teats are eight. The young are of a light brown above, and have the hairs on the tail proportionally longer and a little more spreading. In September they moult, and assume a dark brown fur on the upper parts.

The moult of the adults takes place in the beginning of summer, and is generally completed by the middle of June, when the colour of the upper parts is darker, and the hair of the feet and under part of the tail brownish-white and silky.

Small individuals, found when the tints of the fur are faded, might seem to belong to Mr Jenyns's sorex rusticus. I have seen many such specimens; but still there is a distinct species, race, or variety, which occurs with us, and apparently in equal numbers. It does not differ materially in colour from the other; but may be distinguished by its smaller size, narrower head, longer and broader snout, somewhat stronger feet, and proportionally longer and thicker tail, which is four-sided, as in the other, but has the hairs a little spreading.

Our country people name sorex tetragonurus the *Thraw Mouse.*

Sorex tetragonurus, Jen. Ann. Nat. Hist., i. 423.
Sorex araneus, Jen. Brit. Vert. Anim., 17.
Sorex araneus, Bell, Brit. Quadr., 109.
Sorex araneus, Penn. Brit. Zool., i. 125.

2. *Sorex Rusticus.* Field Shrew.

Dusky brown above, brownish-grey on the sides, greyish-white beneath; feet very small, with short hairs; tail more than half the length of the body and head, four-sided, narrower at the base, of nearly equal breadth throughout, depressed toward the end; flat beneath, covered with short, slightly-spreading hairs, forming a pointed terminal tuft; upper canine tooth bilobate, with the lobes obtuse, the anterior considerably longer; first and second small molar tooth nearly equal, and level with the basal lobe of the canine tooth; third much smaller; fourth somewhat less; fifth very small, inconspicuous, partly from being placed a little within the line of the rest; lower canine tooth directed forwards, and with three semicircular lobes on the edge, gradually decreasing from the front; teeth tipped with reddish-brown.

As the description of this species would be in most respects the same as that of Sorex Tetragonurus, it will be enough here to point out the circumstances in which they agree and differ. They are both dusky brown above, greyish-white beneath, and brownish or reddish-grey on the sides; the number and form of their teeth is the same; their feet and tail are similar. When their pile is new, they are of a deep brown above, and the tail is well covered with fine hairs, and those at the tip are elongated, so as to form a pointed pencil; but when their pile is old and worn, they are of a much lighter tint, greyish-brown or reddish-brown above, and the lighter colour of the sides is not then so contrasted with the darker colour of the back.

(*To be continued in our next Number.*)

Transition Rocks (Palæozoic Rocks of Messrs Rogers) of North America.

The following interesting document is extracted from an address delivered at the meeting of the Association of American Geologists and Naturalists, on May 4. 1844, by Professor H. D. Rogers. It contains a short account of the order of succession, and some of the characters of the different North American transition formations, from the deepest or oldest, called *Primal*, which rests upon certain sandstones or quartz rocks (one of them called Potsdam sandstone), and conglomerates without petrefactions, which may be considered as the uppermost deposits of the primitive class. To those reading on American Geology, this sketch will prove very useful. The nomenclature part of the extract does not harmonise with our views on this subject.

We propose to distribute the whole great body of strata from the base, that is, from the *Potsdam sandstone*, and the conglomerates, to the top of the coal measures in *nine* distinct *series*, the products of as many great successive *periods;* and resorting to the analogy between these periods and the nine natural intervals into which the *day* is conveniently divided, we have named them in ascending order, the *primal*, *matinal*, *levant*, *premedidial*, *medidial*, *post-medidial*, *ponent*, *vespertine*, and *seral series*, the deposits of the dawn, morning, sunrise, forenoon, afternoon, sunset, evening, and twilight periods of the Great Appalachian Palæozoic day. Subdividing each *series* in obedience to natural and obvious relations of the organic remains and mineral boundaries, we have named each ultimate subdivision or *formation*, calling the time during which each formation was produced an *epoch;* and between the series and formations, we have constructed *groups*, in all cases where the natural affinities of the formations require that two or more of these latter shall be united into associations, subordinate to the series.

Our *Primal series* embraces the four great rocks between the base of the Palæozoic strata and the base of the first limestone, the calciferous sandstone of New York. Of these, the primal white sandstone would seem to be the only formation existing in New York, or, according to Owen, on the north-western margin of the basin in the western states.

The *Matinal series* includes all the strata from the horizon of the base of the calciferous sandstone, to that which marks the top of the Hudson River slate in New York, and the top of the blue limestone of the western states. This series, in south-western Virginia, and East Tennessee, embraces a thick and important middle group, consisting of three formations, not extending north-east of the New River, and only imperfectly represented in some portions of the western states.

The *Levant series* includes all the formations between the horizon, terminating the Matinal rocks, and one running through the top of the water lime formation of New York, the top of the non-fossiliferous and "pitted rock" of Lake Huron, and through a plain low in the cliff limestone of the western states. It takes in, therefore, the Medina, Clinton, Niagara, and Onondaga salt groups, and water lime of the New York survey.

The *Premedidial series* embraces the strata between the top of the *water lime* and the top of the *Oriskany sandstone* of New York, and includes, therefore, the *Pentamerus* and *Catskill* Shaly limestones of that state, as its oldest formation, and the Oriskany sandstone as its newest; and, besides these, a middle formation not there seen, but well developed in Pennsylvania, with characteristic fossils.

The *Medidial series* ranks in it all the strata between the top of the Oriskany or Premedidial sandstone and the *Marcellus black* slate of New York, or the black bituminous slate of Ohio, Indiana, Kentucky, and Middle Tennessee. It, therefore, includes the *Schoharie grit* and *Onondaga* and *corniferous* limestones of New York, and the upper division of the cliff limestone of the west.

The *Postmedidial series* embraces that very natural assemblage of formations, commencing with the black slate just named, and crossing with the horizon which marks the base of the *Catskill red sandstone.* It contains, therefore, for New York, the Marcellus shades, the Hamilton group, the Tully limestone, the Genesee slate, the Portage group, and the Chemung group, and for the west all the strata between the top of the cliff limestone and the bottom of the carboniferous limestone.

The *Ponent series* includes all the rocks between the base of the Catskill red sandstone and the top of the overlying conglomerate. (Formation X. of the Pennsylvania and Virginia Annual Reports.) It usually embraces but two formations, the Ponent red sandstone, and the Ponent conglomerate, though the former of these requires for some districts a triple subdivision.

The *Vespertine series* comprehends the interesting formations above the horizon of the Ponent conglomerate, and below that at the base of the great conglomerate under the coal measures. In Pennsylvania, it is composed of the thick red shale deposit of the coal regions; and in Virginia, of a much more complex set of strata, including a lower red shale or variegated marl, next a great thickness of carboniferous limestone, and then an upper set of shales with alternating sandstones. In the western states, on the other hand, it consists almost exclusively of the carboniferous limestone and its subordinate chert.

The *Seral series* embraces one vast and multiform body of coal strata, the thickness of which in Western Pennsylvania and Virginia, exceeds three thousand feet, being in the anthracite basins probably still greater. The lowest or oldest subdivision of this series, is the Seral conglomerate, and the true coal formation overlying this, is divided into four distinct members,—the *older coal measures*, *older shales*, *new coal measures*, and *new shales;* these last terminating the entire succession of one thick and wide-spread Appalachian strata.

The whole body of rocks here grouped into nine series, contains, upon the most careful analysis which we have been able thus far to institute, about forty-eight formations, few, if any, of which are co-extensive with the present limits of the great Palæozoic basin in which they lie, or even with that part of it included between the Blue Ridge chain, the Mississipi River, and the great Lakes. Those which were the most widely deposited, are the Matinal magnesian limestone, the Levant older (or Niagara) limestone, the Vespertine (or carboniferous) limestone, and the older coal measures. Others occupy a relatively circumscribed area, yet none are called formations which are not the products of distinct formative actions operating during epochs characterised by distinct groups of races.

My brief limits will not allow me to present here even the general scheme of names by which we purpose to designate the divisions of this extensive system of strata; but I will explain succinctly the principles upon which the names are chosen. The title given to any formation is composed, first, of the name of the *period* to which it appertains; and, secondly, of a word or words descriptive of the *ruling mineral character* of the rock; and to these is appended, when we wish to specify the type under which the formation is referred to, the name of the district or place where it is so developed. Let me exemplify this by one or two instances. The well characterized formation, called in the New York survey, the Marcellus shales, is named by us the *Postmedidial older black slate*, while the Genesee slate is called *Postmedidial newer black slate*, and a member of the Clinton group of New York, occurring there as a thin bed of brown and ponderous sandstone (seen on the Sequoit), but expanded in Pennsylvania and Virginia into an important mass, having characteristic fossils, and a maximum thickness of two hundred feet, we propose to call the *Levant iron sandstone.*—American Journal of Science and Arts, Vol. xlvii.—No. 1, p. 154, July 1844.

On the Biluchi Tribes inhabiting Sindh in the Lower Valley of the Indus and Cutchi. By Captain T. Postans.*

Communicated by the Ethnological Society,

The general term of *Bilúchi* is applied to a race professing the Mahomedan religion, whose country is hence called Bilúchistan, which may be described as the whole of that mountainous and desert region stretching westward of the Indus from Cape Monze to the Valley of Shawl, and of which Kelat may be considered as the capital. This people thus form a connecting link, as it were, between the Persian and

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* Read before the Ethnological Society on the 10th of April 1844.

Affghan tribes beyond, and the mixed Rajput races who occupy the northern and north-western portion of Guzerat in India.

The earliest detailed and well authenticated account given, I believe, of this people by a European authority, is by that distinguished traveller, and now high functionary, Sir Henry Pottinger, who, in the year 1810, undertook a highly dangerous though deeply interesting journey through the whole extent of this country, and has recorded, in a series of valuable notes, the result of his personal observations and inquiries. From that period up to the last five or six years, few, if any, Europeans have had the opportunities of seeing more of them than was presented by casual journies through portions of their country: of such the most interesting results have been given by Mr Masson, who, taken as a traveller beyond the Indus and in Central Asia generally, is probably the most valuable as an actual authority, from the intrepid manner in which he threw himself amongst these wild and lawless people, and the favourable opportunities he therefore had, for a long period of time, of intimately studying their peculiarities and characteristics. These few observations are made at starting, lest any undue value should be placed on the slight remarks which are now to be made, and which solely have for their object the results of the author's experience of portions of tribes, with many of whom the British Government has for the last five or six years been unexpectedly brought more or less into contact, seldom amicably, and latterly in deadly hostility, and over many of whom prospectively it intends, it is to be hoped, to extend the fostering hand of civilization and take to its charge, along with the millions who own its sway in the vast regions of the East. And here the author trusts he shall stand excused if, in having the gratification and advantage of addressing a Society like the present for the first time, he ventures to offer his humble, though sincere, tribute of congratulation, that a body so formed should exist in this country, having such laudable objects to work out as increased knowledge of the races, states, and condition of that " noblest work of the Creator," in all parts of the globe; for surely few purposes for which societies are formed can be considered as of greater interest or even merit; and, as applied to that magnificent portion of British dominion, to which all the attention that this great nation can shew, will but be found barely inadequate to do justice. Inquiry into its vast and ever varying population must be highly valuable, if the result be only to bring us more intimately acquainted with a people, who demand not only an interest excited by curiosity, but to whom this nation individually and collectively are under deep responsibilities; for it may, in all reverence, be fairly inferred, that so immeasurably important a charge as providing for the well-being of a large share of the population of the globe, was not committed to us as a nation by Providence, without demanding a due weight of obligation.

The origin of the Bilúchis, as a distinctive class, is involved, like most inquiries of this sort in the East, in obscurity; though it may be conjectured that they are of an Arabian stock, and in all probability came to the neighbourhood of the Indus, either shortly prior to or at the period of the first Mahomedan conquest eastward under the Khalifat of Walid. Their own traditions vaguely ascribe their original locality to Shám or Damascus, though they have no date or record, oral or inscribed, to attest it. As, however, the seat of the Khalifat was in those days at Damascus, and it was from thence that the army which conquered the countries bordering the lower Indus was dispatched, there is some reason for concluding that they were colonies from these conquerors who either subdued and possessed themselves of the countries of the aborigines, who were Hindus, driving them out or else causing them to be amalgamated in religion with themselves by conversion, of which certain classes amongst them to this day bear considerable evidence.

Such are the Bábis in higher Bilúchistan, and the Jutts in the lower country. It is also particularly noted by the Mahomedan historians of that period, that certain tribes (an appellation not applicable to Hindus, but which the Moslems adopted,) embraced Islamism, and were obedient to the conqueror, receiving immunities for so doing. A list of these tribes is even given. But to the Bilúchis. They are certainly a different race from all about them, they hold no affinity except in religion with the Affghans, who are more of the Persian character, and are again distinct from the Brahims and Mekranis, who are farther west. The true Bilúch, or, as he proudly styles himself proudly, the "Usul," (literally, originally pure,) Bilúch of the desert is decidedly a particular and distinct class, and possesses peculiarities apart from his geographical position, which would appear to mark him as having considerable claim to an original offshoot from the Arabian family. With respect to the claims of these people to a Jewish origin, it may be said, like those of the Affghans, to consist principally in the conformation of feature,—the division into tribes and certain curious adherence to the Levitican law in the brother marrying the brother's widow,—punishment of adultery by stoning to death, and other minor points. This is too interesting a subject, however, to be passed over lightly, but where conjecture can only be applied, and where, moreover, the strong bias of the mind might lead to erroneous conclusions, in default of anything authentic, it is perhaps better to dismiss it than to hazard mere opinions. Suffice it, therefore, to observe, that the Bilúchi cast of feature is certainly Jewish, the appearance and costume of the wilder tribes, such as is strikingly represented in Calmet's Illustrations of Patriarchal Habiliments (though it may probably arise from the same causes of climate, &c.) and that, as before observed, several of their laws, social and religious, bear an affinity to those of the Levitican;—but whether they (the Bilúchis) have any claim to be of the lost tribes, in any Jewish extraction beyond an Ishmaelitish one, is a subject requiring deeper and more learned antiquarian research, than has hitherto been applied, and until competently dealt with, had better be left alone.

The history of the Bilúchi is as much involved in obscurity as their origin, until a certain period, when they appear to have constituted themselves with the Brahors under Nasir Khan, about the middle of last century, an independent people, and Kelát became if not the seat of regal power, at least of a powerful chieftainship, which the various tribes duly acknowledged and maintained by a complete system of feudalism. As our object is, however, rather to inquire into the present condition of this people, as presented to our view, than to discuss points which may be considered after all to have secondary or antiquarian interest, we may proceed to describe the Bilúchis as they are, or lately were, for with many of them a new order of things has arisen within these two years, and causes are at work which may possibly have a great effect ultimately on their moral and social condition.

The first great feature of the Bilúchis, is their intricate division and subdivision into numerous *Koums* or tribes, and these again are subbivided into almost endless families or minor parties. Each tribe acknowledges implicitly the authority of a chief, which office is hereditary. The attachment, amounting to devotion, paid by this people to their chiefs, is manifested on every occasion whether of peace or war, and a true patriarchal system is thus perpetuated. But the tribes are by no means unanimous amongst themselves; on the contrary, it is difficult to find any two who are not at feud with their neighbours, and a great many have blood quarrels, which are perpetuated by continued acts of violence, for a blood feud can never sleep, and it is said that a Bilúch never foregoes his revenge, though for mutual advantages these feuds sometimes slumber, and are relinquished for a certain period, and seasons are agreed upon between parties for mutual advantages, wherein they abstain from violence ; but on the expiration of these, the old state of deadly animosity is revived with increased bitterness, and a condition of society therefore exists, which is analogous to that of the Arabs and wild tribes of other countries. This, however, does not prevent this people from amalgamating to meet a common foe ; their private sources of quarrel are in such cases kept in abeyance, and as a proof of this, the British troops in the course of our campaigns beyond the Indus, often found that Bilúchi tribes, who were well known to be at most deadly feud with each other, had joined in meeting the British bayonets in the various terrific defiles and passes which the Bilúchis held as their own unaleniable right and property.

There are no less than fifty-eight distinct tribes branching from three great heads, *Rinds*, *Mughsees*, and *Nihrões*, not calculating their subdivisions, enumerated by Sir Henry Pottinger. Of the numbers of these it would be difficult to arrive at anything like an approximation, but of those located immediately on or near the banks of the Indus, it was calculated that 40,000 fighting men could be collected, and late events have proved that this was a pretty fair estimate of their strength, though this, it must be remembered, refers only to those dwelling in the cultivated plains, and not including those of the desert or the mountaineers. The principal tribes located in Sindh are the *Murris* (in reality a hill tribe, but having colonies in the plains), *Khosas*, *Muzaris*, *Mughsis*, *Umranis*, *Lakis*, *Chandiers*, *Julbanis*, *Jatois*, *Salpúrs* (the late reigning chiefs were of this family), *Kainas* (the preceding dynasty who appear to have been rather of a sacred stock than Bilúchi), *Rinds*, *Búrdis*, Kurmatis, Jokias, and Numrias (two tribes inhabiting the range of hills immediately to the westward of Karuchi, and belonging in reality to the province of Lus, under the dominion of the Jam of Beila), though their services as escorts to the trader and traveller are constantly called for through Lower Sindh, and others. Of these the *Rinds*, *Búrdis*, *Muzaris*, *Umranis*, and *Jatois*, are found to have their head-quarters in the partially desert tracts lying between the Indus and the Bolan Pass, and in or near the same locality are also found the *Murris*, *Brogtis*, *Dúmkis*, *Jekranis*, and *Jekrarús*. The *Chandias*, again, are in the Chandokah district, of which Larkhana is the capital, and which is notorious as being the most fertile in all Sindh; a very powerful and numerous tribe, whose influence, when thrown into the balance, has often helped to settle matters affecting the stability of the rulers. There is another very important tribe, the *Lagharis*, the chief of whom, Ahmed Khan, was a distinguished nobleman and statesman at the Court of Hyderabad, holding an office equivalent to that of vizier or prime minister, but this tribe is said by some to be of Jutt extraction, and not real *Bilúch*. The Khosas were formerly a powerful tribe, but attaching their fortunes to the falling house of Kalora, they were visited, accordingly, by the successful Talpúrs. On the confines of the desert known as the *Thurr*, which separates Sindh from Cutch and Guzirat, they are predatory, but in Sindh are cultivators and peaceable, and for Bilúchis, an industrious class. I am not aware that there are any physical peculiarities distinguishing tribes generally, though, as will be hereafter noticed, the desert and hill Bilúchis differ in costume, stature, and habits from their brethren of Sindh. There are numerous other tribes in the line of country designated, but they scarcely merit detail, were not the materials wanting.

The Bilúchis, in their divisions into tribes, have a great deal of the family pride which distinguishes the Rajpút; and of the above heads of families, the *Rinds* are considered to hold a particularly high place—many of the other tribes, therefore, claim *Rind* extraction, such as the Murris, Dúmkis, Jekranis, and others. In marriage this is particularly observable; a daughter may be given by a Rind to a *Rind*, but it would be considered degrading to marry into a lower order of clan, the extreme pride with which this people boast of their claim, as before observed, to "*Usul.*" A real unmixed Bilúchi blood is peculiar, and seldom seen amongst Mahomedans in the East, though happily for them they are, or pretend to be, totally ignorant or unmindful of the very low estimation in which, as a people, they are elsewhere held, the term Bilúch being by the other inhabitants of these countries literally translated by its initial Persian letters to mean ج bé, thus ب *bud* or bad ل lám, *lorchur* or vagabond, and چ chccm, *chóz* or thief, a silly invention in itself, but significant of the bad character this people have gained.

The Bilúchis located in Sindh, acquired under the late Bilúch dynasty a great and important share of the country, as Jabgirdars and feudatories, the tenure by which they held their possessions being military service, and very analogous to the old feudal system in Europe. This also obtained throughout the whole of Bilúchistan. Locating themselves in the plains, and on the banks of the river, the Bilúchis in Sindh, though wild and barbarous as compared with the inhabitants of our own Indian provinces, were yet superior in this respect to their untamed brethren of the desert and mountains, who, occupied alternately as robbers or shepherds, are as wild as it is possible to find any race of men similarly situated. Even those who may be considered as peaceable clans, since they occupied themselves on their farms or estate as supervising cultivators, carried with them the thieving propensities for which they are notorious, and thus acquired for the inhabitants of Sindh generally a proverbially dishonest title, though in reality it appears to be this class which alone merited it; but we shall refer to this point more particularly in discussing the character of this people.

(*To be concluded in our next Number.*)

SCIENTIFIC INTELLIGENCE.

METEOROLOGY.

1. *Climate of Kordofan.*—The climate of Kordofan, says Ignatius Pallme in his travels, is very unhealthy, especially during the rainy season; no hut is then, indeed, to be met with, in which there are not, at least, several sick. In the dry season, again, all disease disappears; at this time, however, not only man, but all living creatures, suffer from extreme heat. The eye then rests with melancholy on the desolate and parched plains, trophies of the victory of the heat over animated nature, where nothing is to be seen but bones of men and animals bleached by the burning sun. During the whole of this season, which endures about 8 months, the sky is clear and cloudless, and the heat is insupportable, especially in the months of April and May. From 11 o'clock A.M. to 3 P.M,, when the thermometer stands in the shade at 38° or even at 40° Reaumur (117° to 122° Fahrenheit), it is impossible for any breathing creature to remain in the open air. Every living being, both men and cattle, with equal eagerness, seek the shade to protect themselves from the scorching rays of a fierce sun. Man sits, during these hours, as if in a vapour bath; his cheerfulness of disposition declines, and he is almost incapable of thought; listless, and with absence of mind, he stares vacantly before him, searching in vain for a cool spot. The air breathed is hot, as if it proceeded from a heated furnace, and acts in so enervating a manner on the animal economy, that it becomes a trouble even to move a limb. All business ceases, every thing is wrapped in a sleep of death until the sun gradually sinks, and the cool air recalls men and animals again into life and activity. The nights, on the other hand, are so sharp, that it is necessary to be more careful in guarding against the effects of cold in this country, than in the northern parts of Europe; for the consequences frequently prove fatal. During the dry season, every thing in nature appears desolate and dismal; the plants are burnt up; the trees lose their leaves and appear like brooms; no bird is heard to sing; no animal delights to disport in the gladness of its existence;

every living being creeps to the forest to secrete itself, seeking shelter from the fearful heat; save that, now and then, an ostrich will be seen traversing the desert fields in flying pace, or a giraffe hastening from one oasis to another. "When I arrived at Lobeid," says the writer, "I only found one single European living, Dr Iken, whom I have before mentioned, a native of Hanover, who, like most of the Europeans, after a short residence there, paid his tribute to the climate."

HYDROGRAPHY.

2. *Depression of the Caspian.*—The President of the Geographical Society of London, in June last, read the note of a Russian operation for determining the actual depression of the Caspian Sea below the level of the Mediterranean—which operation had been reduced by the eminent astronomer, M. Struve, then in England, and communicated by that gentleman to him. A few years ago it was generally believed that the waters of the Caspian were at least 300 *feet* below the level of those of the Black Sea and Mediterranean. This view was adopted in consequence of a series of barometrical observations; but it having been found that, from the great number of stations across the land separating the Caspian from the sea of Azoff, small errors had become greatly magnified, a new survey was made. Three able mathematicians, Messrs Fuss, Savitch, and Sabler, were, therefore, employed to make independent trigonometrical levellings; and their observations agreeing to within a foot or two, give, for the mean result, 83.6 *English feet* as the depression, the possible error being limited to 1.3 foot, which definitively settles this long pending geographical question.—*Athenæum*, No. 870, p. 601.

3. "*The Calling of the Sea.*"—As the foreknowledge of approaching changes in the weather is of importance, especially to fishermen and agriculturists, I invite attention to a very common, but not generally known, indication of such changes.

In Mount's Bay, and probably in all places similarly situated, there is often heard inland, at a distance from the shore, a peculiar hollow, murmuring sound, locally termed "the calling of the sea," which, if proceeding from a direction different from the wind at the time, is almost always followed by a change of wind, generally within twelve, but sometimes not until a lapse of twenty-four, or even thirty hours. It is heard sometimes at the distance of several miles, although on the shore from which it proceeds, the sea may not be louder than usual; and yet at other times, even when the sea on the shore is louder than usual, and in apparently equally favourable states of the atmosphere, it cannot be heard at the distance of a mile. When the sound, in fine weather, proceeds from the coves or cliffs on the west or south of the observer, it is followed by a wind from about west or south, accompanied generally with rain. When it comes from the east or north of the observer, a land wind from about east or north succeeds, attended with fine weather in summer, and often with frost in winter. All my own observations during the last twelve months, confirm the above statement; indeed, none of those of whom I have inquired, and who have for many years been accustomed to observe these indications, can recollect a single instance of their failure. This sound must not be confounded with that arising from a "ground sea," which is the well known agitation along the shore, occasioned by a distant storm, and which may likewise often proceed from the direction subsequently taken by the wind, for this latter noise propagates itself in every direction, and chiefly in that of the wind; whereas the "calling" is heard only from one direction, and usually contrary to the wind. Besides, if this "calling" come from the north-eastern, or inmost shore, of the bay, and the wind afterwards change to that quarter, it could not possibly arise from a "ground sea" produced by a distant storm from that direction.

Hence it appears that the "calling" of the sea depends not on the condition of the sea, but on that of the atmosphere. I am informed, too, that previously to a change of weather, all distant sounds are heard loudest in the direction which the wind subsequently takes. The fishermen of Portleven, who are very observant of all signs of atmospherical changes, are particularly attentive to this. They also notice the motion of the clouds, and observe whether these are moving or not in the direction of the vanes—one very singular and sure sign which they have, that the wind will change in the course of the day to the south-west, is a morning fog flowing from the Loo-pool into the bay towards that point. These last indications may possibly assist in ascertaining the cause of the "calling of the sea."—Richard Edmonds, Esq. *Eleventh Annual Report of the Royal Polytechnic Society of Cornwall*, p. 47.

GEOLOGY.

4. *Fossil Physeter Whale.*—In the collection of Mr Brown of Stanway, is a remarkable fossil, which Professor Owen proved to be the tooth of a Cachalot, and, in the report of the British Association for 1842, states to have been procured from the Diluvium of Essex. Mr Charlesworth having examined the specimen in question, considers it a genuine crag fossil from the same deposit with the cetacean remains described by Professor Henslow at a previous meeting of the Association.

5. *A recently discovered Bed of Diamonds in Mexico.*—According to the report of an expert geologist, Von-Gerold, diamonds have been discovered in the great Mexican mountain range, in the *Sierra madre*, in the direction of Acapulco, (to the S.W. of the city of Mexico.) Humboldt had conjectured that diamonds and platina occurred further to the N.W. in the gold washery of Sonora. It is also said that immense tracts of auriferous alluvium occur in Upper California, as also in New Mexico. They are principally in the possession of wild tribes, a circumstance which will accelerate the intrusion of the North Americans, and hasten the taking possession of them by strangers.*

6. *Structures and probable mode of formation of the older Mountain Rocks.*—Those naturalists who continue to believe in the existence of the true stratified structure in primitive and many transition rocks—who consider mineral veins as of after formation to the rocks in which they are contained—who conceive inclined tabular rocks (the old stratified rocks of authors) to have been upraised from an original horizontal position—who maintain that the natural seams of rocks, sometimes bounding or enclosing masses many hundred yards or fathoms in extent, are me-

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* Poggendorff's Annalen, vol. 62, p. 283.

chanical, and not chemical effects—who deny the existence of colossal distinct concretions, and see the columnar structure of trap, porphyry, granite, &c., as a mechanical effect—who believe all conglomerated rocks to be of mechanical formation—that all sandstones are of mechanical origin—that trap rocks have received their structure and positions through volcanic agency—that granite, syenite, and porphyry, are of after formation to the rocks with which they are connected; and that the apparent fragments these rocks sometimes contain, are mechanical effects—will obtain clearer views of the structures of the crust of the earth, and their mode of formation, if they reject the mechanical hypothesis, and call to their aid the chemical and contemporaneous geognostical doctrine.

7. *Do fossil organic remains of living animal species occur in any of the four great classes of rocks, viz. Primitive, Transition, Secondary, and Tertiary?*—At present, it is a prevailing opinion, that fossil remains of living animal species occur, along with those of extinct species, in rocks, as in those of the tertiary class. If, however, the late discoveries of palæontologists prove true, it follows, that all the fossil animal species, even those found in the newest of the tertiary class, are extinct; and that fossil living species do not occur but in alluvial deposits.

8. *Diluvium and Alluvium.*—As the waters of the globe formerly covered its whole surface, and as these waters have gradually sunk to their present level, or rather as the land has gradually risen above the surface of the ocean, does it not follow that the formation of diluvium and alluvium commenced about the same time, and that both must occur in every country, the diluvium being formed by the action of the sea, at whatever height it may be found above its present level, while the alluvium is the result of the action of the atmosphere, lakes, rivers, and springs?

9. *Geology of Abyssinia.*—Captains Galinier and Ferret, who are about to publish at the expense of the minister of war, an account of their expedition to Abyssinia, have sent to the Academy of Sciences a list of the subjects treated of in the four volumes of which their work will consist. They have also transmitted a separate memoir on the geology of Tigré and Sémen.

For the purpose of drawing up this geological description of Abyssinia, and forming the map and geological sections which accompany it, the authors have collected on the spot a series of specimens, which are now deposited in the Jardin-des-Plantes, illustrated by sections and a number of notes. Their work shews that Abyssinia is a country which has been subject to many geological accidents, and that these accidents are of different kinds. In consequence of this complex orography, we perceive from the first, that this country is necessarily very remarkable in respect to its geological constitution. MM. Galinier and Ferret are convinced, that if Abyssinia does not offer the complete series of formations to the geologist, it presents at least a great number, and that it ought to be ranked among the most complex and remarkable countries in a geological point of view. The formations they have met with in Abyssinia, range from the first to the last degree of the geological scale. Thus they have found primary and transition formations in the country of the Chohos, in Tigré, &c.; secondary formations at the extremity of Tigré and the country of the Taltals; tertiary and modern formations on the shores of the Red Sea, in Tigré, Sémen, Chiré, &c. If we join to this range of the geological scale, a great variety of sedimentary rocks, rocks of plutonic and volcanic origin, also those usually named metamorphic, a considerable number of extinct volcanoes, hot springs, repositories of iron, sulphur, rock-salt, combustible substances, malachite, &c., we will obtain a general idea of the geological importance of Abyssinia.

The general character of the relief of this country has been produced by three kinds of ridges. The first is formed by the direction of the mountains of Tarenta; it affects the primary formations, and runs from the north-west to south-east, that is to say, in a direction parallel with the Arabian gulf. The second is represented by the mountains of the transition formations of Tigré; it evidently runs from the north-east to south-west, that is, in a direction parallel to the shores of the Gulf of Aden in the Indian Sea. Lastly, the third is formed by the general direction of the tertiary plateaux of Tigré, Chilé, Agamé, &c.; and runs from north-north-west to south-south-east.

It would be very interesting to connect the three systems of elevation which MM. Galinier and Ferret have pointed out in Abyssinia, with the general systems of elevation established by M. Elie de Beaumont; but for want of sufficient data, the authors of the memoir have not attempted to do this. With regard to volcanoes, MM. Galinier and Ferret have observed, that the products of numerous volcanoes are to be seen in the islands near Abyssinia, and on the shores of the sea. They consider it probable that they existed before the period when the Ptolemies founded establishments on the coasts; but it is not certain that any now exist in Abyssinia; the springs of hot water they observed on the shore, could not decide the question, although some reach 64° and 65°,* and even exceed that. Many travellers, it is true, affirm that they have seen volcanoes on the coasts of Choa. M. Rochet, in particular, has mentioned the volcano of Dofano, and figured it as being in a state of activity; it is situate in the vicinity of Angobar; but MM. Galinier and Ferret are of opinion, that it is very possible M. Rochet has mistaken fumaroles for a true volcanic eruption; for the Abyssinians appear to have no idea of volcanoes.†

10. *Limestone of Corfu and Vido.*—Captain Portlock, R.E., is of opinion that the limestone of Vido is probably oolite.

11. *Geology of Malta and Gozo.*—From the observations of Lieut. Spratt, R.N., it appears that the Maltese islands are formed of tertiary strata, of the Middle or Miocene period.

12. *Rocks of Tangier.*—In the travels of *Badia*, better known as *Ali Bey*, occurs the following passage:—

" The ground which forms the basis of the coast at Tangier, is composed of different beds of secondary granite, of a compact or fine granular texture. These beds are inclined to the horizon, and form with it an angle of 50 to 70 degrees. They are generally one foot and a half thick; their direction runs from east to west, and their inclination, by which the angle is formed, is northerly.

" The distance between the beds is commonly about two feet, and this space is filled with a sort of white, and not very hard clay, which, taking the same direction, forms intermediate beds of a slaty texture. These beds of granite and clay are very little above the level of the sea."

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* It is not mentioned whether of Reaumur or the Centigrade.

† L'Institut, No. 547, p. 210.

All this is erroneous. Not a vestige of granite occurs in the country around Tangier. I visited that country in 1814, and passed along the coast from the east of Tangier towards Cape Spartel, where I observed no other rock than sandstone, and a crumbling clay-slate, or rather shale. In my journal is the following note:—

"Observe that the rocks under the walls of the town consist of alternate strata of a yellowish-brown sandstone, of a fine grain, and a crumbling bluish slaty clay. The beds of the former are about a foot and a half thick, the latter generally three or four feet. This alternation is continued under the town, and forms the rock on which the castle stands."

I traced it in various other places on both sides of Tangier; and Cape Spartel seems to consist of this sandstone. The other indications of granite, mentioned by Ali Bey, between Tangier and Tetuan, I believe to be erroneous; for the whole of that part of Morocco appears to be exceedingly like, in geological structure, to the opposite coasts of Spain, which, westward of Tarifa, are composed of sandstone. In Barbary, these rocks seldom attain an elevation of above 30 or 40 feet along the coast, until it rises rather abruptly into the rugged and lofty limestone mountains, opposite to Gibraltar, named Apes' Hill by the English, the *Mons Abyla* of antiquity—the geological characters of which seem identical with the limestone of Gibraltar, and the eastern Sierras of Andalusia.—T. S. T.

13. *Eruption of Vesuvius of* 1843.—M. Rozet reported to the Geological Society of France, at one of its last meetings, some circumstances relative to an eruption of Vesuvius, which he witnessed in September 1843, and which he had leisure to examine in all its details. His report mentions many facts of a certain degree of importance in explaining volcanic phenomena in general.

About the middle of July 1843, the holes of the crater of Vesuvius were all stopped, small columns of smoke issued here and there by small fissures, and there was a large prominence near the northern edge of the bottom, on which it was easy to walk, although the smoke issued by numerous crevices. On the 30th September, an immense quantity of fumaroles, much more considerable than those of the Solfatara, arose on all the edges of the crater, the interior walls, and even as far as the summit of Palo. These fumaroles issued by fissures and holes, some of which were covered with a pale yellow crust of muriate of iron, others surrounded with a white efflorescence of nearly pure marine salt. The smoke was composed of steam, with a small quantity of hydrochloric acid, easily known by its pungent odour, but there was so little of it, that the observer could remain in the midst of the smoke for upwards of five minutes, without being put to much inconvenience No trace either of sulphur or of sulphureous odour was perceptible. The bottom of the crater was covered with fluid lava, whose black and extremely irregular surface presented numerous fissures, in which the red matter, in a state of fusion, was perceptible. This surface was smoking, principally from the fissures; and the smoke, much thinner than that from the walls of the crater, appeared to be composed of nearly pure steam. Towards the northern edge of the bottom, a black cone, pierced with two mouths, almost diametrically opposite to each other, rose to a height of from 25 to 30 yards above the liquid lava. From each of these mouths a thick column of smoke and jets of melted matter continually issued; the column of smoke was traversed by an incandescent mass, projected into the air, which soon fell back again in a shower of fire. At intervals of 30 seconds, a dull noise was heard in the interior of the cone, and immediately a sheet of melted matter arose with a loud detonation, from one or other of the mouths alternately; it fell back again in plates round the opening. At the same instant, a jet of fragments of various sizes was projected into the air, and rose to a height of 30 or 40 yards. Having descended to the bottom of the crater, the observer, seated at a distance of 50 yards from the southern mouth, could see pretty distinctly what was passing in its interior, to a depth of two yards. The detonations which succeeded the internal noise were stifled, and about as loud as those of a 12-pounder. At the moment when they took place, the opening of the mouth was very red, but it became black immediately after, reddened again at a new detonation, and so on successively. The jet of matter accompanied the detonation. The smoke which issued from the mouths was red to 3 or 4 yards above the opening; it then became grey. There was no appearance of flame, and nothing indicated the combustion of a gas. During all the time the observer remained in the crater, he did not feel the least shock or trembling of the earth. The matter in a state of fusion did not ascend above the mouths; but at the southwest foot of the cone there was seen a swelling of 2 yards in height and 10 in diameter, covered with a brown cracked crust, under which the burning lava appeared, and from which two small currents issued, advancing so slowly towards the west, that it was impossible to perceive their movements with the eye.—*L'Institut,* No. 547, p. 216.

MINERALOGY.

14. *The colouring matter of Flint, Carnelian, and Amethyst.*—It appears from the experiments of W. Heintz, as stated in Poggendorff's Annalen, vol. 60, stück. iv., p. 527, that flint is coloured by organic matter; but this is not the case with carnelian and amethyst. The carnelian appears to be coloured by iron in the state of oxide; amethyst by iron in the state of acid—the ferric acid.

15. *Composition of the Calc-chrome Garnet, the so called Uwarowite.*—According to Herr Erdmann, in Stockholm, this beautiful garnet is composed of the following ingredients,—silica, 36.93; alumina, 5.68; oxide of iron, 1.96; oxide of chrome, 21.84; lime, 31.63; magnesia, 1.54; oxide of copper, a trace; =99.58.

16. *Beaumontite and Lincolnite identical with Heulandite.*—The Beaumontite of M. Levy, as stated in Silliman's Journal for April 1844, appears to be merely a variety of Heulandite; and in the same journal we are informed that the Lincolnite of Professer Hitchcock is also a modification of Heulandite.

17. *Zeolites.*—*Levyne, Gmelinite* and *Phacolite*, appear to be varieties of chabasite, and not distinct species; and the so-called *Caporcianite* is now believed to be a variety of one of the well ascertained species of zeolite.

18. *Periclase, a new mineral.*—M. Scacchi, Professor of Mineralogy at Naples, has communicated to the *Annales des Mines*, through his friend, M. Damour, a description of a mineral found in the ancient lavas of Vesuvius, of a vitreous appearance, obscure green colour, and confused crystallization, imbedded in a calcareous matrix, like the gehlenite of Fassa. It cleaves readily in three directions parallel to the faces of a cube, whence it derives its name, Periclase. It crystallizes in regular octahedrons; is infusible before the blow-pipe. The powder is entirelt soluble in acids. Hardness equal to felspar. Specific gravity 3.75. It is composed of magnesia and a little oxide of iron. Its composition in 100 parts, is

	First Analysis.	Second Analysis.
Magnesia,	92.57	91.18
Oxide of iron,	6.91	6.30
Insoluble mattter,	.86	2.10
	100.34	99.58

—*Ann. des Mines*, 4th Series, Vol. iii. p. 369.

19. *On Piauzite, a Mineral Resin.* By W. Haidinger, Esq.—The following are the mineralogical characters of this mineral:—

Colour, blackish-brown; streak, yellowish brown; massive; lustre, resinous; fracture, imperfect conchoidal; translucent on the thin edges; mild; feeble lustre on cut places: hardness, = 1.5; specific gravity, 1.220.

The Piauzite, although mild under the knife, is, on account of its low degree of hardness, so feebly coherent, that we can, as is the case with other mineral resins, rub it easily between the fingers. It is traversed by numerous and generally parallel rents.

Chemical Properties.—At 315° cent. it inflames; it burns at a somewhat higher temperature, with a peculiar aromatic smell, with much flame and strong evolution of soot, to ashes. The melting point was determined in a linseed oil bath. It is completely soluble in ether and caustic potash; it is almost entirely soluble in anhydrous alcohol, but less soluble in alcohol containing water. Fuming nitric acid converts the colour of the dark-brown resin into yellowish-brown. Heated in a glass tube, there is distilled from it a yellowish, acid reacting, oily fluid. In its common state it contains 3½ per cent. of hygroscopic water. The dry affords 5.96 proc. of ashes.

Geognostic Situation.—It occurs in veins from one to two inches wide, traversing brown coal, and bituminous wood, in a brown coal deposit, in the neighbourhood of *Piauze*, north from Newstadtl, in Carniola.

Use.—Its easy inflammability, and the abundance of soot it deposits during burning, cause it to be used for giving the black colour to cast-iron ware.—*Poggendorff's Annalen*, vol. 62, p. 275.

ZOOLOGY.

20. *Early History of Guano.*—Sometime ago Mr Teschemacher made an interesting communication to the Boston Society of Natural History,

on the origin of the valuable manure called guano, from the sea islands off the coast of Peru. We extract the following passage from it:—

With reference to the opinion entertained by some, that the guano had been accumulating from a period perhaps prior to the origin of the human race, Mr T. translated the following passage from the "Memoriales Riales" of "Garcillasso de la Vega." Lisbon, 1609, p. 102. "On the sea-coast, from below Arequipa as far as Tarapaca, which is more than two hundred leagues of coast, they use no other manure than that of marine birds, which exist on all the coast of Peru, both great and small, and go in flocks perfectly incredible, if not seen. They are reared on some uninhabited islands which exist on that coast, and the manure that they leave is of inconceivable amount. At a distance the hills of it resemble the mounds on some snowy plain. In the time of the Incas there was so much vigilance in guarding these birds, that, during the rearing season, no person was allowed to visit the islands under the pain of death, in order that they might not be frightened and driven from their nests. Neither was it allowed to kill them at any time, either on or off the islands, under the same penalty." Each district or territory also had a portion of these islands allotted to it, the penalties for infringement of which were very severe. From this extrardinary care, it is probable that the Incas did not permit any remarkable consumption of this valuable manure beyond the annual addition; and the consumption during the depopulation of South America by the Spaniards, could by no means have equalled those annual deposits. Even the greatest thickness of seven to eight hundred feet might, without extravagant calculation, be deposited in about three thousand years, at the rate of two or three inches a-year. The feathers do not appear different from those of birds of the present day. Mr Blake, a member of our society, who has visited these deposits, has a shell found in the guano, very much resembling the *Crepidula fornicata* of this coast, but not in any way fossilized. On this coast it never rains, so that the deposits of manure are not, like those on other coasts, annually washed away.—*American Journal of Science and Arts*, Vol. xlvi. No. 1. p. 203.

22. *On the Hyæna.*—The traveller, Ignalius Pallme, in his Travels in Kordofan, vindicates the hyæna from the charge of ferocity and cruelty, usually brought against it by writers of Natural History, most of whom assert that the animal is untameable. He says:—

"In the court of a house at Lobeid I saw a hyæna running about quite domesticated. The children of the proprietor teased it, took the meat thrown to it for food out of its jaws, and put their hands even into its throat, without receiving the least injury. When we took our meals in the open air, to enjoy the breeze, as was our general custom during the hot season, this animal approached the table without fear, snapped up the pieces that were thrown to it, like a dog, and did not evince the slightest symptom of timidity. A full-grown hyæna and her two cubs were, on another occasion, brought to me for sale; the latter were carried in arms, as you might carry a lamb, and were not even muzzled. The old one, it is true, had a rope round its snout, but it had been led a distance of twelve miles by one single man without having offered the slightest resistance. The Africans do not even reckon the hyæna among the wild beasts of their country, for they are not afraid of it."—*Athenæum, No.* 872, *p.* 641.

List of Patents for Inventions granted for Scotland from 23d June to 21st September 1844, inclusive.

1. To Walter Frederick Campbell of Islay, Esquire, in the county of Argyle, Scotland, "an improved rotatory engine to be driven by steam or other power."—25th June 1844.

2. To Robert Foulerton of the Jamaica Coffee-House, Cornhill, in the city of London, master mariner, "certain improved machinery for moving vessels and other floating apparatus."—25th June 1844.

3. To Thomas Hancock of Goswell Mews, Goswell Road, in the county of Middlesex, waterproof-cloth manufacturer, "an improvement or improvements in the preparation or manufacture of caoutchouc in combination with other substances, which preparation or manufacture is suitable for rendering leather, cloth, and other fabrics waterproof, and to various other purposes for which caoutchouc is employed."—25th June 1844.

4. To Edmund Morewood of Thornbridge, in the county of Derby, merchant, and George Rogers of Stearndale, in the same county, gentleman, "improvements in coating iron with other metals."—27th June 1844.

5. To George Wilson of St Martin's Court, St Martin's Lane, in the county of Middlesex, stationer, "improvements in the cutting of paper for the manufacture of envelopes, and for other purposes."—27th June 1844.

6. To Robert Davison of Brick Lane, in the county of Middlesex, civil engineer, and Willilm Symington of East Smithfield, in the county of Middlesex, civil engineer, "a method or methods of drying, seasoning, purifying, and hardening wood, and other articles, either in a manufactured or unmanufactured state, parts of which are applicable to the preparation and dessication of animal, vegetable, and mineral substances."—1st July 1844.

7. To William Brockedon of Devonshire Street, Queen Square, in the county of Middlesex, gentleman, "improvements in the manufacture of pills and medicated lozenges, and in preparing or treating black lead."—8th July 1844.

8. To George Edmund Donisthorpe of Bradford, in the county of York, top manufacturer, "improvements in combing wool and other fibrous substances."—8th July 1844.

9. To John M'Bride, manager of the Nursery Spinning and Weaving Mills, Hutchesontown of Glasgow, in Scotland, "certain improvements in the machinery and apparatus for weaving by hand, steam, or other power."—9th July 1844.

10. To **Moses Poole** of the Patent Office, Serle Street, in the county of Middlesex, gentleman, being a communication from abroad, "improvements in the manufacture of paper." —11th July 1844.

11. To **George Miller Clarke** of Albany Street, Regent's Park, in the county of Middlesex, tallow chandler, "improvements in night lights, and in apparatus used therewith."—11th July 1844.

12. To **William Henry Phillips** of Bloomsbury Square, in the county of Middlesex, engineer, "certain improvements in the means and apparatus for subduing and extinguishing fire, and saving life and property, and in obtaining and applying motive power, and improvements in propelling."—15th July 1844.

13. To **Edward Buxton** of Basinghall Street, in the city of London, merchant, being a communication from abroad, "improvements in spinning wool, cotton, and other fibrous substances."—15th July 1844.

14. To **George Gwynne** of Princes Street, Cavendish Square, in the county of Middlesex, gentleman, and **George Fergusson Wilson** of Belmont, Vauxhall, in the county of Surrey, gentleman, "improvements in treating certain fatty or oily matters, and in the manufacture of candles and soap."—22d July 1844.

15. To **David Cheetham** of Rochdale, in the county of Lancaster, cotton-spinner, and and **John Tatham** of the same place, machine-maker, "certain improvements in machinery or apparatus to be employed in the preparation and spinning of cotton, wool, and other fibrous substances.' —23d July 1844.

16. To **John Holland Butterworth** of Rochdale, in the county of Lancaster, cotton-spinner, "certain improvements in machinery or apparatus applicable to preparation machines used in the spinning of cotton and other fibrous materials."—23d July 1844.

17. To **Jacques Bidault** of Paris, in the kingdom of France, merchant, being a communication from abroad, "improvements in applying heat for generating steam, and for other purposes, which improvements may be employed to obtain power."—24th July 1844.

18. To **James Caldwell** of Mill Place, Commercial Road, in the county of Middlesex, engineer, "improvements in cranes, windlasses, and capstans."—24th July 1844.

19. To **James Hardy** of Birmingham, in the county of Warwick, gentleman, "certain improvements in the process of welding tubes, pipes, barrels, or hollow rods of malleable iron, by machinery."—30th July 1844.

20. To **Joseph Hall** of Bloomfield Iron-Works, in the parish of Tipton, in the county of Stafford, iron-master, "improvements in the manufacture of horse-shoe nails."—1st August 1844.

21. To **Lawrence Hill** Junior, of Glasgow, civil engineer, being a communication from abroad, "improvements in machinery for manufacturing shoes for horses and other animals.—1st August 1844.

22. To Charles Low of Robinson's Row, Kingsland, in the county of Middlesex, "certain improvements in the making or manufacturing of iron or steel."—2d August 1844.

23. To William Sutcliff of Bradford, in the county of York, manufacturer, "improvements in preparing, dyeing, sizing, or dressing yarns, and winding yards, and manufactured fabrics of wool, flax, cotton, silk, and other fibrous materials."—6th August 1844.

24. To William Isaac Cookson of the borough and county of Newcastle-upon-Tyne, Esquire, "improvement in apparatus for burning sulphur in the manufacture of sulphuric acid."—8th August 1844.

25. To James Smith of Queen Square, Westminster, Esquire, "improvements in slubbing, spinning, twisting, and doubling cotton, and other fibrous substances."—8th August 1844.

26. To William Losh of Newcastle-upon-Tyne, Esquire, "improvements in the manufacture of metal chains, for mining and other purposes." —8th August 1844.

27. To Henry Bewley of No. 3 Lower Sackville Street, in the city of Dublin, apothecary and chemist, and George Owen of the same place, chemist, "improvements in the mode of confining corks, or substitutes for corks, in bottles and other vessels, whether made of glass, earthen, or stone ware, containing liquids charged or not charged with gas."—9th August 1844.

28. To Anthony Lorimer of Clerkenwell Close, in the county of Middlesex, bookbinder, "certain improvements in the apparatus and means of facilitating drawing from nature or models."—9th August 1844.

29. To Pierre Armand Le Comte de Fontainemoreau of No. 1 Skinner's Place, Sise Lane, City, London, being a communication from abroad, "improved crane, called dynamometric."—10th August 1844.

30. To Pierre Armand Le Comte de Fontainemoreau of the English and Foreign Patent Office for Inventions, No. 1 Skinner's Place, Sise Lane, in the city of London, being a communication from abroad, "a new mode of locomotion applicable to railroad and other ways."—10th August 1844.

31. To Alexander Ewing of the town of Dumbarton, Scotland glass splitter, "certain improvements in the manufacture of crown glass." —14th August 1844.

32. To Arthur Wall of Bisterne Place, Poplar, in the county of Middlesex, surgeon, "certain improvements in the manufacture of steel, copper, and other metals."—16th August 1844.

33. To Stephen Hutchison of the London Gas Works, Vauxhall, in the county of Surrey, engineer, "certain improvements in gas meters." —16th August 1844.

34. To Joseph Martin Kronheim of Castle Street, Holborn, in the city of London, engraver, being a communication from abroad, "improvements in stereotyping."—3d September 1844.

35. To **Robert Ferguson** and **John Clerk**, both of the city of Glasgow, in the county of Lanark, "an improvement in printing and calendering."—4th September 1844.

36. To **James Pillans Wilson** of Belmont, Vauxhall, in the county of Surrey, gentleman, "improvements in treating fatty and oily matters, and in the manufacture of candles."—4th September 1844.

37. To **Francois Stanislas de Sussex** of Bethnal Green, in the county of Middlesex, chemist, and **Alexander Robertson Arrott** of Torrington Square, in the same county, chemist, "improvements in the recovery of manganese, used in making bleaching powder."—4th September 1844.

38. To **James Smith**, late of Deanston, now of Queen's Square, London, civil engineer, and **William Gairdner Jolly**, residing at Endrick Bank, parish of Drymen, and county of Stirling, Scotland, "certain improvements in the form of tiles for draining, in implements for manufacturing thereof, and in the modes of manufacture."—4th September 1844.

39. To **John Lionel Hood** of Old Broad Street, in the city of London, gentleman, being a communication from abroad, "an improved composition, or mixture of metals, applicable to the manufacture of sheathing for ships and other vessels, bolts, nails, or other fastenings."—9th September 1844.

40. To **Peter Ward** of Oldbury, in the counties of Salop and Worcester, late of West Bromwich, in the county of Stafford, practical chemist, "an improvement in combining matters for washing and cleansing."—12th September 1844.

41. To **Edwin Sheppard** of Manchester, in the county of Lancaster, builder, "certain improvements in machinery, or apparatus for planing, sawing, and cutting wood, and other substances."—13th September 1844.

42. To **John Beare** of St John's Wood, in the county of Middlesex, civil engineer, "certain improvements in engines or machines for raising or conveying water and other fluids."—18th September 1844.

43. To **James Petrie** of Rochdale, in the county of Lancaster, engineer, "certain improvements in steam-engines."—19th September 1844.

44. To **William Newton** of the office for Patents, 66 Chancery Lane, in the county of Middlesex, civil engineer, being a communication from abroad, "certain improvements in treating and preparing oil or fatty matters."—20th September 1844.

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ERRATA IN VOLUME XXXVI.

Page 312. The letter P. in Fig. 1 is entirely misplaced: it should be at the intersection of the lines IL and NE.

Page 316, lines 33 and 35, *for* DNQY *read* DNGY.

ERRATA IN PRESENT VOLUME.

Page 135, bottom of page, *for* panleontological *read* paleontological

„ 182, line second, *for* intoxicating *read* unintoxicating

" 226, line 7 from bottom, *dele* " If, as it has been suggested," and line 6 *dele* " then"